THE LIFE

OF

JOHN HUNTER.

By JESSÉ FOOT, SURGEON.

SINE IRÂ ET STUDIO, QUORUM CAUSAS PROCUL HABEO.

TACITUS.

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ADVERTISEMENT.

I HAVE arranged this Performance into the four following Parts—beſides the Introduction—in order to procure, a Reſt, for the Reader—and to eſtabliſh, for myſelf, a Syſtem in the Execution of it.

London, Dean-ſtreet, Soho,
April 6, 1794.

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INTRODUCTION.

HE who engages to write the life of an eminent professional man, when he has closed an active scene, and descended into the vale of eternal rest, must be considered as attempting an important undertaking, increasing the stock of human information, and furnishing another example for directing the conduct of those now walking upon the face of the earth, and of ages yet to come.

It does not necessarily follow, that he alone is an object for historical inquiry, who has been eminent in dispensing an increase of happiness to mankind, conferring useful knowledge in a greater

proportion, or carrying the grand career of genius into practice: the best and brightest examples of men should alone be the models for imitation; but yet it will be ever found necessary, for the purpose of inculcating their true value, to form critical comparisons with characters that have betrayed a contrary inclination.

The progressive improvement of science cannot be more purely marked, nor the particular faculty of him who is supposed to have been of importance in its cause, cannot be better ascertained, than at the moment when his powers are ceasing to act, and his personal influence is withdrawing. This is the immediate point of time which is most favourable for shewing, what has been done on the same given subject by others, and what was done distinctly by him; what was known before, and in his own time to others, and what was made known distinctly by him.

The historian is better prepared for doing an act of justice, for demonstrating truth to the public eye, and must necessarily expose himself to detection by living witnesses, if he aim to pervert either. He cannot plead ignorance from want of information; he cannot say, that his documents have been carried down the rolling current

rent of time, and swallowed up for ever in the gulph of oblivion; with his memory fresh, and his attention awake, he can neither be forgetful of the professional points then acquired by another, nor lessen the value of discoveries made before the year 1794, by what might be in progress hereafter. The advancement of science will be more accurately traced, by noting periods distinctly; and that fair title to his own invention, which every man of genius has the right of claiming, will not be so liable to be confounded nor impeached.

It is a lamentable consideration, that characters desirable to posterity should be so commonly deferred, from a habit of indolence, a system of delicacy, or a motive of fear. Prominent features, which can be but barely recollected, are thus defaced; and the memory being incapable of recalling what has never been deeply impressed, admits of fabulous conceits, which are thus so frequently found to be intruding upon the places of truth.

An historian of the time present rarely likes to be seen unravelling a character, for the purpose of demonstrating what has been with studied art concealed; of distinguishing betwixt an

inclination for fame, by direct or indirect pursuits; he will shrink from the office at the very time he should have undertaken it, and more especially, when the splendour of reputation intimidates his mind, increases his awe, and mocks his resolution. But this is the very time that independent judgment would decide for the challenge of inquiry; whilst the impression is yet fresh, which reputation has made upon opinion, whilst the undertaking cannot be said to be less just, because it is bold; nor less honest, because it is conscious.

There will always be found a ready race of panagyrists, who may be said to be distinct from the true historian; who draw their portraits in miniature, and are smooth and flattering, telling rather what a man ought to have been, than what he was. These are so little resembling the originals, as to entitle them to no rank in the scale of common humanity; both incapable of informing, or instructing, these seem to have no relative concern with the business of life, doing no credit to the head, nor honour to the heart, neither promoting the cause of truth, nor advancement of knowledge. These are found to be so useless, as rarely to command the shortest notice; and therefore, amidst the thousands that have been pro-

produced, but few have exceeded the life of an ephemeron. Their common lot is an unbeneficed oblivion, neither finding their way into any public libraries, nor becoming tenants of the attention of mankind, as long as tomb-stones of the same personages in church yards.

Of the professional life of JOHN HUNTER, whose celebrity hath attracted my attention in common with the rest of the world, I shall not open the account with a boast of uncommon encomium. I must be content with telling, that I write more to inform than to praise, more for example than glory; that I intend to reason from consequences, rather than strike the mind with splendid attractions of admiration for the character I am about to display.

Heroics are extremely well suited to the design of him, who is undertaking to draw the character of a warrior, whose renown in battle has struck terror among the inhabitants of the globe; and who has spread desolation wherever he was engaged: there heroics must necessarily be the substitute for truth, whenever he experiences the common lot of human nature. But to trace improvements in the arts of anatomy and surgery, to examine into the result of mental appli-

application, in a pursuit connected with philosophic study, to penetrate into the concealed designs of the heart, and watch its bias, to investigate pre-determined points, and separate them from the open effusions of genius, requires firmness in mind, not volubility in words, settled axioms of truth, not copious streams of fiction.

This undertaking does not consist in a detail of resources in the mind, for obtainment of power, or aggrandizement of fortune; but is chiefly confined to the close investigation of the progress of anatomical and surgical improvements. I am therefore bound to establish my plan upon justice. The charge which I have taken upon myself stands exactly upon a similar basis of a judge, who never acquits nor condemns any one from the narrow motive of partiality, but every one is treated according to the nature of his case.

Nor does it follow, that because I undertake to write the professional life of John Hunter, I am indulging myself upon a subject most suiting to my will, or complying with the bent of my most favoured inclination. I can obtain by this no reputation I had not earned before; I can display no power opportunity had not given me; the

the subject admits of no rhetorical decoration, which my ambition or imagination ever thirsted after. I might be in truth only considered to be pursuing what I began, only compleating what I undertook in his life time, from an intention of doing justice to my own understanding, when some, who were older than me, declined it ; and the younger dared not attempt it. And I will not presume, but assert the privilege of exercising the result of a professional education, and abiding the test of solid judgment in an inquiry after truth.

To allay the tender apprehensions of those, who plaintively expressed their fears and anxieties for me, and who persuaded me to decline the work ; to enlighten the blind admiration of those who never having read a single line he has written, believed him to have been the first Surgeon of his time ; and to inform the implicit, but zealous pupil, who relying upon the truth and integrity of his master, without consulting his own understanding, was persuaded, that the latest discoveries, and newest opinions of John Hunter, could not be found already registered in former authors ; this professional life, if I mistake not, will be found to be not badly calculated.

Thus

Thus as far as they carry conviction to any man's understanding, my labours may be of use; beyond the evidence it carries with it, I advise him not to follow any man's interpretation.

PART

PART I.

FROM THE TIME OF HIS STUDY IN THE SCHOOL OF ANATOMY, AND CONSEQUENT TRANSACTIONS, TO THE YEAR 1760.

IT is not my intention to enter into a minute account of the life of John Hunter; nor to pretend to be nicely accurate in the dates of thoſe domeſtic tranſitions, from childhood to that period which terminates the career of human action.

John Hunter was a younger brother of the late Dr. William Hunter, and was born in the county of Lanerk in Scotland, ſome time about the year 1728.

Nothing that has reached my knowledge till lately, has been ſaid of the tranſactions of his youth. And as I do not aſpire after adding another inſtance to the natural deſire in man for propagating wonders, I ſhall ſuppoſe that when John Hunter was in the arms of his nurſe, he was ſeen exactly like any other child in a ſimilar ſituation; that he was not diſcovered in performing any of thoſe romantic feats, which have been

ſaid, by the ſecond ſighted, to be precurſors of future great atchievements; that he was neither detected in playing with a ſerpent, thruſting his hand into the mouth of a lion, nor ſtaring the keen eyes of the eagle through and through, till he forced him to blink at his own, the keeneſt. Nor ſhall I attempt to amuſe with any anecdotes of young Hunter, during his ſcholaſtic education; whether his genius was ſo unbridled and overbearing, as not to be brought to ſubmit to the trammels of diſcipline; whether from that time he had fixed the determination, never to read, which he has been declared to continue during his latter days; nor whether he had any education, excepting ſuch as thoſe have, who are bound apprentices to a common trade.

A wheel wright or a carpenter he certainly was, until the event of William Hunter becoming a public lecturer in anatomy, changed the ſcheme of his future occupations, and determined him to accept the invitation of his brother: to lay down the chiſſel, the rule, and the mallet; and take up the knife, the blow pipe, and the probe.

The firſt profeſſional performance of his, was preſented to the public eye in William Hunter's Medical Commentaries; as if it had been written

ten by John Hunter in the year 1756. But whether it was publiſhed in any other way, before it appeared in the Commentaries, I cannot tell. At any rate, it was ſaid to be drawn up by John Hunter, if not then publiſhed, in the year 1756, in juſtification of ſome diſputes about anatomical diſcoveries, then agitated, and vehemently conducted betwixt the two Hunters, and the three Monroes of Edinburgh, the father and two ſons. Theſe diſputes extended to three different diſcoveries, and involved in them beſides, the illuſtrious De Haller of Gottingen, and the late Percival Pott.

The particulars of theſe diſputes I ſhall proceed to diſcuſs, in order to diſcover what was the ſhare that John Hunter took in them; what was the diſplay of moderation and genius, which he had ſhewn when in the vigour of youth, in the opening of his underſtanding, and in the dawn of his introduction to that anatomical theatre, where he has been ſuppoſed ſince to have acted, from the ſuperior powers of the mind, a part ſo conſpicuous, as to become in the end the firſt anatomiſt, and firſt ſurgeon, in the world.

In the year 1746, William Hunter ſucceeded Sharpe, in reading a courſe of lectures on anatomy and ſurgery, to a ſociety of navy ſurgeons,

at a houſe ſomewhere in Covent Garden; in which he gave not only that ſatisfaction, which fairly promiſed to him the future fame he acquired as a lecturer, but alſo induced him to extend the plan of his lectures, ſo as to bring it to be a perfect ſchool of anatomical inſtruction.

It was about this time, that John Hunter entered into the diſſecting room of his brother, when he was about eighteen years old: and I have no reaſon to doubt, but he was of great ſervice to his brother, and in as ſhort a time as any young man with the ſame education could have been. That he was always of a turn to induſtry, is very clear; and that the purſuits of anatomy are not much retarded by the want of education, may be believed; as whatever was of value that treated on anatomy, has been ſoon tranſlated into Engliſh; ſo by this, he was enabled, with the aſſiſtance of his brother, to ſelect the ſubject which claimed his cloſeſt attention, whenever the ſame was then attracting the attention of anatomiſts in other countries.

His mind was led with the eaſieſt inclination, to purſue that ſtudy his inquiries had approved; and with probably more ſincerity and ardour, than if his education had been of a more liberal nature. He had found an unexpected path, directly

rectly leading to fame, opened before him, and that without another choice; for an uneducated man is undoubtedly reſtrained in his election of the occupations of life; he cannot wander without a guide; ſome one there muſt be to inſtruct him; and John Hunter, thus conditioned, wiſely availed himſelf of his brother's aſſiſtance, for conducting him to eminence in the profeſſion of an anatomiſt.

As ſoon after his initiation to the anatomical ſchool, as the progreſs in ſcience will ever permit, he was found to be conducting ſuch experiments, as tended to be uſeful in the lecture room; and to be advancing the knowledge of ſome anatomical doctrines, which, at the ſame point of time, were occupying, or had occupied, with an equal degree of diligence and fervor, the attention in the anatomical ſchools of Gottingen, Berlin, Leyden, and Edinburgh.

The principal ſubjects of inquiry, which arreſted the attention of all the anatomiſts of that time, were

1. Of the Injections of the Teſtis:

2. Of the Origin and Uſe of the Lymphatic Veſſels and Abſorbents by Veins:

3. Of

3. Of the Diſcovery of the Hernia Congenita.

I do not profeſs a deſire to be eſteemed by thoſe philoſophers, who call themſelves the ſoundeſt, from having diſcovered that all the actions of mankind tend to a neceſſary uſe: as if man were not endowed with a conſcience, and had not a diſcretionary privilege of diſcerning right from wrong. Naturaliſts may tell us, if they pleaſe, that in the operations of nature, there is nothing uſeleſs; and, as the aſſertion is general, I have no deſire to diſturb it by contradiction. The plea of neceſſity would be but poor, when urged by one, who contradicts another without his improving the ſubject which gave cauſe for the contradiction. Whenever the intention is juſt, reaſon will not permit the will to be inactive.

Theſe reflections ariſe from a ſtrong indignation againſt thoſe who preſume, that becauſe men differ in opinion, they muſt neceſſarily be turbulent in their diſpoſitions; as if it were impoſſible for a difference in opinion to be conducted by reaſon; or for a ſpirit of emulation to be raiſed, without a conſequent deſtruction of thoſe who engage in it. The praiſe or blame ariſing out of every diſpute is relative in its nature, and ſolely

ſolely dependant on the motive for which it was purſued.

I ſhall explain theſe diſputes in the order I have placed them, by beginning with that on the injections of the teſtis; and which originated with William Hunter, in the manner that it is ſeen by the account of it which appeared in the Critical Review for November 1757, under the title of "Facts relating to the Diſpute between Dr. Hunter and Dr. Monro," and to which I refer my readers, or to Dr. William Hunter's Commentaries.

I. OF THE INJECTIONS OF THE TESTIS.

WILLIAM Hunter has there aſſerted, that ſome time, as a week or a fortnight, after this firſt public demonſtration, his brother, John Hunter, made a trial of injecting the teſtis, and it ſucceeded. And here we behold the firſt fruit of this young anatomiſt, offered up as a ſacrifice at the ſhrine of diſputation; which was only the beginning of many more conteſts, and which gave cauſe to an anatomical war upon paper, not finally concluded during five years—half the time of the ſiege of Troy.

From the ſtatement of the caſe, as given there by the parties, I ſhall directly proceed to draw my concluſions.

It is of the utmoſt importance, for aſcertaining the degree of ability in any man, to diſtinguiſh betwixt an invention, which is the mere reſult of induſtry, and one which is ſolely dependant upon the powers of the imagination.

The ſubject which claims my preſent inveſtigation, could not have been decided by the mere exerciſe of the imagination; but it muſt have

have been firſt ſuggeſted, and afterwards proved by experiment.

The ſuggeſtion of it in thought was no mark of genius in any one; it was a common and familiar piece of knowledge, which required the art of injection to demonſtrate; and that demonſtration was the only difficult obſtacle in the queſtion.

Every one muſt have known the purpoſe of the teſtis, and that what was elaborated by it was in coitû diſcharged through the penis. The common operation of ſtoning lambs, to prevent them hereafter from copulating, proves the knowledge to be in vulgar practice. Therefore, the moſt important object of injecting the teſtis, was nothing more than ſurmounting a difficulty, by demonſtrating what was already known, its tubular vaſcularity. Who firſt undertook to make the experiment by injections, who firſt ſucceeded in it, and who firſt imparted that ſucceſs by publication;—this was the pith and marrow of the contention.

It appears, by the anſwer of Donald Monro, that his father, the profeſſor, had proſecuted the idea as far back as the year 1747; and that de Hailer had not only been proſecuting the ſame

idea by experimental injections, but had alſo given the reſult of them in the Philoſophical Tranſactions; and that profeſſor Monro had likewiſe given the reſult of his ſucceſs in the Medical Eſſays.

It appears, that the idea was not only firſt ſuggeſted by de Haller and profeſſor Monro; but that experiments were commenced by them before William Hunter had began to teach anatomy, and before the brother John had began to learn it.

It appears, that William Hunter was far behind the two profeſſors, de Haller and Monro, in beginning to prove, by ocular demonſtration, a connection of the ducts, coming out of the teſtis, to form the epididymis: as in a note annexed to the evidence of Henry Watſon, for confirming the complete preparation of an injected teſtis, ſhewn by William Hunter in the autumn courſe of the year 1752, he ſays, "I take the opportunity with pleaſure of doing this gentleman the juſtice that I did at my lecture, with regard to his obſervations upon the teſtis, by declaring, that he firſt ſhewed me the ducts coming out of the teſtis to form the epididymis in a preparation where he had traced them by diſſection with great accuracy."

It

It appears, that, after the ſuggeſtion of the idea of injecting the teſtis, and after the experiments to confirm it had been made, and were making by de Haller and the Monroes, the Hunters were active and indefatigable in the ſame proſecution; and that they ſucceeded as early, as thoſe who had the advantage of them, by beginning ſooner.

It appears, that young Monro, when he announced by publication his ſucceſs, in completely injecting the body of the teſtis, was not apprized of the like ſucceſs by any other anatomiſt: as the firſt, ſuppoſed to have been ſo completely done by William Hunter, had not been divided, and conſequently not proved; and as the event of the ſucceſs of the ſecond, done by John Hunter, had not come to his knowledge, when he publiſhed concerning the ſucceſs of his own injection. But whether the Hunters had ſucceeded in both their injections, or neither, it matters not in the queſtion before us, in point of right or reaſon. If both the Hunters had ſucceſsfully injected, and if both had confirmed it; if the Monroes, both profeſſor and ſons, if de Haller alſo, nay, if all the profeſſors in Europe had been apprized of the fact, yet the two preparations belonging to them could not have produced a third, belonging to another.

Theſe two preparations were not Monro's; he had made that which he aſſumed the right in, he had not only made it, but had demonſtrated it; and having ſo done, he had the common right of publiſhing it, without the leaſt imputation of theft, and of which he was accuſed by the Hunters.

It appears, that the Hunters, who of all men cannot, nor ever could have been ſuſpected of being negligent in maintaining their anatomical rights, nor diffident in meeting the eye of the public, have not only in this diſpute, but throughout their lives, founded their complaints upon an unreaſonable plea; whether intentionally, or from a weakneſs in their nature, I will not haſtily decide. They have uniformly expreſſed themſelves, upon every cauſe of diſpute, (and the whole of their hiſtory is compoſed of diſputes,) as if they conceived that, what had been read or demonſtrated by them in the lecture-room, was virtually and actually publiſhed; that it was, *bona fide*, equal to, and had all the right and effect of an open publication.

What they were deſirous of being conſidered as a publication, was only a demonſtration. A preparation cannot be publiſhed, but a plate from it may. They both ſeem to be convinced of

of the impoſſibility of uniting the differences upon certain occaſions: as whenever they found it their intereſt not to be reduced to the neceſſity of ſo feeble a reſource in argument, as whenever they choſe to claim a right, which they could claim by common means, they always diſcovered ſtrong ſigns that they did know there was a palpable diſtinction betwixt demonſtration in a lecture-room, and a printed publication: for no anatomiſts have publiſhed more than the Hunters; and no anatomiſts have ſwelled their publications with ſuch varieties of anatomical plates, not only taken from the original preparations, which they demonſtrated, but alſo from the appearances of parts which they had diſſected; and to demonſtrate which in their ſucceſſive courſes, if they had not thus publiſhed their plates, they muſt have diſſected the ſame over and over again.

It appears, that there was a trial of the art of injecting the teſtis, purſued in common, by de Haller, the Monroes, and the Hunters; and that the ſucceſs in it was become proper to de Haller and the Monroes, by their publications: but that the Hunters, until they came forth with their complaints of what others had done, had never publiſhed. But if the Hunters had publiſhed their ſucceſs in the experiment ever ſo late,

late, without impeaching that which fairly belonged to de Haller and the Monroes by their having publiſhed more early; if they had left their reputation for diligence in anatomical purſuits to ſtand upon the merit it really poſſeſſed, they would not have been undervalued; nor, for the firſt time, expoſed themſelves to ſtrong ſuſpicions of their naturally poſſeſſing the torment of jealouſy; the ſtrongeſt ſymptom of a weak underſtanding.

And laſtly, it appears, now the art of injecting is improving, that the difficulty of ſucceſs is only ſurmounted, by attending to the proper ſubject for injection: and that it ſhould be only attempted on a teſtis of a ſubject which was in a ſtate of perfect health, and whoſe age was favourable for a perfect ſecretion of ſeminal fluid; who had not undergone emaciation from ſickneſs, and whoſe ſeminal veſſels had been emptied a ſhort time before his death; and that it ſucceeds to a greater certainty on the teſtis of a large animal than a ſmall one.

Before I cloſe this ſubject, I beg to be indulged in beſtowing my tribute to the memory of Henry Watſon, in whoſe collection I have ſeen many injected teſtes. He paid his laſt debt to nature a few days after John Hunter. And

it

it is due to his reputation, that his name ſhould find a record in this page, leſt his modeſt merit might have otherwiſe, paſſed away in ſilence.—He was Surgeon to the Middleſex Hoſpital, at its firſt commencement; but reſigned that for the Weſtminſter Infirmary, at which he continued to the day of his death. Some time ago, being rendered infirm, through a paralytic ſtroke; his end was haſtened by the alarm of a fire in Rathbone-place, in the vicinity of his houſe. He died very far advanced in years.

He had formerly read lectures on anatomy in the Borough, and poſſeſſed a very extenſive well choſen collection of anatomical preparations. He was Fellow of the Royal Society, and publiſhed many papers in the Philoſophical Tranſactions, and in the London Medical Journals. He had been a very good operator, and a ſurgeon of ſound judgment; very eaſy of acceſs, and modeſt in his communications.

He was one of the examiners at the Surgeon's Hall. When in his duty, he never contracted the frowning brow, to confound the diffidence of youth; but by the placidity of his demeanour, ſolicited a diſplay of the knowledge they poſſeſſed. He had a conſiderable taſte for muſic, and filled up his leiſure hours with the ſolace of harmony.

2. THE

2. THE DISPUTE BETWIXT THE HUNTERS AND MONROES, ON THE ORIGIN AND USE OF THE LYMPHATIC VESSELS, AND OF ABSORPTION BY VEINS.

THE part which John Hunter appears to have taken in the diſpute upon the preſent ſubject, is, by his attempting, out of date, to prove, by experiments made on five animals, that there was one, and but one ſyſtem of veſſels for abſorption. But if theſe experiments, made by him, had come forth before the publication of young Monro upon the ſame ſubject, he would have then produced them in proper ſeaſon, for entitling him at leaſt to a ſhare of claim in the diſcovery, properly belonging, ſo far as it relates to the Hunters, to Monro ſolely.

Theſe experiments on five living animals were began by John Hunter, in November 1758, and finiſhed in Auguſt 1759: they are concluded with the following emphatic words: "Here is a new doctrine propoſed in phyſiology, viz. that the red veins do not abſorb in the human body."

On the appearance of young Monro's treatiſe, the conduct of the Hunters went no farther than to ſay, that they had taught, at their lectures,

lectures, what young Monro had pretended to diſcover: and that what he had publiſhed as his own diſcovery, they had not only taught, but he had ſtolen from them. They did not then ſay that they had publiſhed any thing upon the ſubject, as they moſt aſſuredly had not; but they relied upon the plea they had before been in the habit of practiſing, namely, that what they taught and demonſtrated in their lecture-room, was virtually equal to a printed publication.

But it does not yet appear, that the Hunters were ſerious in this aſſertion; for if they had been, John Hunter would not have gone about experiments, beginning in November 1758, and ending in Auguſt 1759, and by them to prove to the world, that he knew what young Monro claimed as his diſcovery, before young Monro publiſhed it,—and to prove that he knew what young Monro had publiſhed in the beginning of the year 1757, by what he had diſcovered by experiments conducted betwixt November 1758 and Auguſt 1759.

Strange as it might appear, this is the ſort of argument offered in their vindication by the Hunters. And by their argument, it appears, that if they had been the authors of the diſ-

covery claimed by Monro, but which had been ſtolen from them, why had they not proved their title in the faireſt way, and made it out in an intelligent manner? Whereas the experiments of John Hunter contradict their own aſſertions; as they were made to aſcertain the truth of young Monro's diſcovery, nearly two years after his publication of it. So that the triumphant words with which his experiments are concluded, at ſo late a ſeaſon as Auguſt 1759, when compared with the reſult of the knowledge imparted by the publication of young Monro's treatiſe, in the beginning of the year 1757, appear to have loſt all the effect of that ingenuity which, among men of erudition, commands a competent admiration.

In the Critical Review for September 1757, the reviewers concluded their account of Dr. Monro's treatiſe, *De Venis Lymphaticis Valvuloſis*, with very ſevere remarks. Theſe were made evidently with the concurrence of the Hunters. For the charge of the Hunters, and the reply of profeſſor Monro, I refer my readers to that Critical Review, or to William Hunter's Commentaries.

Profeſſor Monro has there ſaid, that what John Hunter and his brother have done, ſince the

the beginning of the year 1755, when young Monro's theſis was publiſhed, declaratory of the publication that followed in the beginning of the year 1757 at Berlin, is out of the preſent queſtion.

But leſt the reader ſhould be at a loſs to know what John Hunter was doing all this while; and how it came to paſs that the ſubject either ſlept with him, or that he was diſtanced in the race he attempted to run with the young Monro, I think I can ſatisfy him as to that point, by proving, from William Hunter's own words, that John Hunter would have proſecuted the inveſtigation ſooner, if he could; and that he poſitively did not proſecute it, notwithſtanding any aſſertion by them to the contrary.

" I had made," ſays Dr. Hunter, " no ſatisfactory obſervations upon the lymphatic glands for ſeveral years after I had read lectures, and therefore never took upon me to decide between Nuck and Ruyſch, whether they were cellular or only vaſcular. All this, as well as the manner in which the lacteals and lymphatics paſs through them, I profeſſedly gave from authors, and not from my own obſervations. My brother found out, to the beſt of my recollection, in the year 1753, or 1754, that he could fill theſe glands uniformly, and the lymphatic veſſels go-

ing from them, by puſhing a pipe into their ſubſtance, as Dr. Nicholls had done in the teſtis. When examined in this way, they have exactly the appearance that Nuck deſcribes. After I had ſeen this experiment repeated to my ſatisfaction, I mentioned it in my lectures, and then confirmed what Nuck had ſaid, from my own obſervation. Having found out ſo eaſy a method, my brother then intended to have diſcovered or aſcertained the ſtructure, and, if poſſible, the uſe of the lymphatic glands; to have traced the lymphatic veſſels all over the body, and to have given a compleat deſcription and figure of the whole abſorbing ſyſtem. This he propoſed to accompliſh, as his other employments ſhould permit. He occaſionally filled theſe glands with air, with mercury, and with ſoft wax. They always appeared to be cellular, and the lymphatics to paſs through them in the manner that was commonly ſuppoſed. To ſee more exactly how theſe things were, he injected ſome with wax, and then ſteeped them in ſpirits of ſea-ſalt for corroſion; but he learnt nothing of them by this experiment; for in waſhing they all crumbled to bits, not only the ſuppoſed cellular part, but the viſible branches of the lymphatic veſſels: which was occaſioned, as he imagined, by the frequency or number of valves in them, interſecting the column of wax. As he wiſhed not

to

to be anticipated, I treated the ſubject lightly at my lectures, and to the beſt of my remembrance, only mentioned his manner of filling the glands, and the eaſy method of raiſing the veſſels wherever there are ſuch glands, and his opinion of the thoracic duct climbing ſo far as the upper cava, inſtead of terminating immediately into the lower, viz. that the chyle was carried a great way before it was poured into the blood, probably for the ſake of being firſt mixed with almoſt all the lymph of the body. Both theſe obſervations I made as from my brother, when Dr. Monro attended me; and when the hurry of diſſections was pretty well over in the ſpring, my brother ſat about a preparation, which he propoſed as a baſis for his intended deſcription and figure of the abſorbing ſyſtem. Dr. Smith of Oxford happened to be in town at that time, and being much pleaſed with the intention and with the preparation, was frequently in the diſſecting room while my brother was diſſecting, and while Mr. Riemſdyk was making the drawing: ſo were many gentlemen of our acquaintance, beſides ſtudents.

"In that preparation and figure, the lymphatic veſſels from the ham upwards to the thoracic duct were ſeen, as well as the inguinal and lumbar glands, all the larger lacteals at the root of the

the meſentery, the receptaculum chyli, (or what is ſo called) and the thoracic duct, all I ſay, finely filled with mercury. So far my brother had gone. A very indifferent ſtate of health, the effect of too much application to anatomy, which obliged him to be much in the country, other unavoidable avocations, Dr. Meckel's publication upon the lymphatic glands, and a diſlike of having any diſpute with Dr. Monro, which, by his father's letter in the Critical Review ſeemed to be threatened, all theſe things, I ſay, have from that time made him lay aſide the ſcheme; and he will hereafter finiſh it, or not, as he may think proper."*

By this quotation, it clearly appears, that the Hunters were never prepared with their figures for publication; and that Nuck, Mekel, de Haller, and Monro, had anticipated all their procraſtinated projects, and made diſcoveries for them.

From the true ſtatement of the queſtion, one can hardly find a ſingle juſtifiable cauſe of complaint from the Hunters; and one is at a loſs for a reaſon, why the appeal to the public was ſo clamorouſly made, for what had been done by others; whilſt the choice was open to them, for doing what they pleaſed, and for exhibiting their drawings

* Hunter's Medical Commentaries, p. 34—35.

drawings when they pleaſed. I ſhall not dwell upon the inſidious manner, by which they deſigned to reſerve their information, as they have betrayed of themſelves as much of that part as I am deſirous of being made known. But ought inveſtigation to wait upon their leiſure, and be ſuſpended by the reſt of mankind, for them to reaſſume it whenever they choſe? Is nothing right or juſt, which is performed by another, when it had not previouſly their approbation? If any production anticipated them, the cry directly was, that they were foreſtalled; if any anatomical queſtion was agitated by them, and another at the ſame time, the cry directly was, that they were robbed; or if their names were not mentioned by an author, although it were impoſſible for the author to know that they were engaged in the ſame purſuit, or even their ſentiments about the ſubject he had written upon, the cry directly was, that they were moſt ſlightingly and indecently treated.

Proud as we are of the inventive faculties of our countrymen, the inveſtigation of the abſorbent ſyſtem is much more belonging to foreign anatomiſts than our own: and I am afraid almoſt to aſſign my reaſon for the ſentiment; but I believe it to be a juſt one. I believe that the abilities of Europeans in general are equal; and

and that the ſucceſs of the foreign anatomiſts beyond our own, is not derived from ſuperior faculties, but becauſe they can afford to purſue a ſtudy, which requires the uſe of time and brings in return no profit, better than we can. The German profeſſors have all of them proviſionary incomes, and are eſtabliſhed by the government with liberal conveniencies.

This obſervation particularly ariſes out of the preſent ſubject; as it is proved to be of that arduous nature, as to have attracted the attention of the moſt eminent anatomiſts for the two laſt centuries.

The perfection of that diſcovery, which aſcertains the ſtructure and uſe of lymphatic glands, which traces the lymphatic veſſels all over the body, which gives a complete deſcription and figure of the whole of the abſorbing ſyſtem in every claſs of animals, and which proves it independent of branches of ſanguineous veins and arteries, brings with it an increaſe of knowledge creative of wonder: it is ſuch a diſcovery, as no ſingle man, endowed with the greateſt patience, ſtrongeſt mind, keeneſt eye, quickeſt ſenſe, and utmoſt longevity, could have compleated.

To

To prove that the ſyſtem of abſorbents muſt have been brought to perfection by the progreſſive advances of many anatomiſts, in many ages, and that it could not have been perfected by any ſingle genius, is eaſily done by referring to the progreſſive hiſtory of the diſcoveries. But independent of this, I might ſay, with a moderate and rational definition of genius;—that, if it conſiſt in an intenſe application of a mind fitting by capacity for this particular ſtudy, and concentrated to this object alone,—ſuch a genius could never have compleated the doctrine of the abſorbent ſyſtem, as we now find it. The nature of the diſcovery muſt pre-ſuppoſe an accumulation of mental ability, a patient exerciſe of human labour.

The lacteals had been ſeen by ancient anatomiſts, before their offices were made known: Herophilus and Eraſiſtratus take notice of white veſſels, but were unacquainted with the uſe of them. Caſper Aſellius, in the year 1622, appears to have been the firſt diſcoverer of the purpoſes of the lacteal veſſels: and ſoon after, when diſſection of living animals became the general practice of anatomiſts, Rudbeck and Bartholine diſcovered the lymphatic veſſels: and this progreſs was farther improved by Picquet,

who diſcovered the thoracic duct, which had been traced before by Euſtachius in a horſe.

So far a foundation of this wonderful ſyſtem in the animal œconomy was laid, when Nuck produced his complete treatiſe on the abſorbent veſſels; and the patient eye of Lieberkuhen diſtinguiſhed the anatomical ſtructure of the *villi* of the inteſtines,—of the beginnings of the lacteal veſſels, from the internal ſurface of the villoſe coat and their orifices;—termed by him, *ampullulæ*: and ſo far as theſe diſcoveries had advanced to his time, I ſhall refer the reader who may be ſolicitous of purſuing the inveſtigation, to the ſeventh volume of the illuſtrious de Haller.

When the ſubject of enquiry had been eagerly conducted by theſe anatomiſts, who may be deemed to be of the claſs, in point of time immediately behind the preſent; among the names of which, I ſhall take notice of Nuck, Ruyſch, Duvernai, Lieberkuhen, de Haller, Cowper, Mekel, Nichols, Akenſide, Monroes, and the Hunters,—all of whom enriched the theory, by partial reſearches into comparative appearances of the ſyſtem of abſorbents, throughout the general race of animals;—it was for a while purſued no farther. By ſome it was diſcontinued, from

from an arreſt by death; by many exhauſted from a tedium of the mind; and by others abandoned, partly from diſguſt, partly from anticipation, but principally becauſe the ſtudy produced no emolument for recompence from failure of ſucceſs, nor reparation for loſs of time. The ſubject appeared to be ſleeping, whilſt John Hunter was upon the wing, to quit the lecture room for the army; and whilſt the brother William found out from inference, that he could employ his time to a better account, both in point of profit and in point of honour :—the laurels which both the brothers gained in this conteſt not being worth the ſcramble they made for them.

I am greatly at a loſs to point out, what advances the Hunters have made in this branch of anatomical ſcience. There is no trace apparent of any thing done by them, but the experiments of the five living animals, made by John Hunter, betwixt November 1758 and Auguſt 1759; and by which, it is ſaid, that he diſcovered the ſyſtem of abſorbent veſſels to be independent of any abſorption by ſanguineous veins and arteries.

If theſe experiments were made at the time they were dated, how came they not to be then publiſhed; eſpecially as the deſign of them was

to juſtify the Hunters, in aſſerting, that what young Monro had publiſhed of the ſyſtem of abſorbents being independent of ſanguineous veins and arteries, was a thought ſtolen from them?—If theſe experiments, made at leaſt eighteen months after the publication of Monro, were not publiſhed as ſoon as they were made, to reclaim that right he had foreſtalled them in—how much leſs could they hope to eſtabliſh that right to themſelves by publiſhing them, for the firſt time, at the time that they did, which was in the year 1764, after John Hunter's return from the war,—five years after they were made,—and ſeven years after the ſuppoſed theft which was the juſtifying cauſe of their being made? And although I have given each of them a niche in the temple which Fame has erected to the progreſſive diſcoverers of the abſorbing ſyſtem, yet my reaſon will hardly ſuffer me even to admit, that what was publiſhed by them on the ſubject, has barely earned the ſituation.

At this period of anatomical hiſtory, when the proſecution of this branch of the ſtudy was ſomewhat ſuſpended by others, Hewſon enriched it by diſcoveries, additionally to thoſe who had gone before him. By innumerable laborious diſſections, inſtituted on living and dead animals, he diſcovered and injected the whole of

of the abſorbent ſyſtem in birds, *amphibia*, and fiſhes; and by theſe important diſcoveries, he proved the univerſality of the ſyſtem of abſorbents; and by demonſtrating their exiſtence in the various claſſes of animals, he rendered it more than probable, that the veins even in them do not abſorb. Theſe diſcoveries of the immortal Hewſon, were conſidered as ſo important in anatomy and phyſiology, that they were rewarded by the Royal Society of London with Sir John Copley's annual gold medal. But his grand career of modeſt merit and patient profeſſional labour, was prematurely ſtopped in the year 1783, when he fell the victim of a putrid fever.

That eclipſe which overſhadowed the anatomical honours of the Engliſh nation from the public loſs of Hewſon, was a ſhort time after diſſipated by the ſucceſſion of Sheldon; the preſent profeſſor of anatomy to the Royal Academy. The laſt work of Hewſon had illuſtrated, by figures, the lymphatic veſſels of the extremities and trunk: but he had left no repreſentation of the lacteals, nor of the lymphatics of the different viſcera of the thorax or abdomen: the lymphatic veſſels of the viſcera, and the lacteals in the human ſubject, were but imperfectly known, even at the late period when we were deprived of him.

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To ſupply this deficiency, Sheldon has corrected the error of former authors, who had publiſhed on this ſubject, and given many elegant figures from nature, of thoſe parts of the ſyſtem in the human body, where they have been falſely deſcribed, and where they have been recently diſcovered. But this does not comprehend the whole of his work; its value is very extenſive; his unreſerved diſcovery of the art of injecting, to which the great progreſs of anatomical ſcience is chiefly indebted, claims a tribute of praiſe for ingenuous candour, and ought, by its example, to induce anatomiſts in future to publiſh, as he has done, whatever they know reſpecting anatomical inveſtigations, which have been derived from injections. The progreſs of the ſcience has been much impeded by a myſtery among anatomiſts, who have generally concealed the compoſition of their injections, and their methods of diſſecting, injecting, and preparing the different parts: a myſtery unworthy the character of a philoſopher.

I am now to tell the reader how the high ſpirit of the ſublime author of "the Pleaſures of Imagination," became ſomewhat ruffled by this diſpute, inſomuch as to have drawn from him a ſmart reply indeed, but ſuch a one as demonſtrated who it was that was called upon in vindication

dication of the feelings proper to a refined ſcholar, and an accompliſhed gentleman:

——— ingenuas didiciſſe fideliter artes
Emollit mores nec ſinit eſſe feros.

Young Monro, in a poſtſcript to his anatomical obſervations, had taken notice of what he called inaccuracies, in a paper publiſhed in the Philoſophical Tranſactions, for the year 1757, written by Akenſide, on the origin and uſe of the lymphatic veſſels of animals. In reply to this, Akenſide publiſhed notes on the poſtſcript, and animadverted ſmartly, and indeed with ſeeming juſtice, on young Monro, for affirming that he only hinted as a conjecture, in the Gulſtonian lecture, what in fact, from premiſes ſupported by argument and experiment, himſelf aſſures us, he deſcribed as the very next thing to a phyſical certainty; and likewiſe for inſinuating, that Akenſide's paper owed its appearance to Monro's treatiſe *de Glandulis Lymphaticis*.

Akenſide evinces, that Monro, in moſt of his remarks upon his paper, either miſunderſtood or miſrepreſented his meaning. In reply to Monro's objection, that the lymphatics are not called veins on account of their valvular ſtructure, but becauſe the fluid in them moves from the ſmaller to the larger branches, and towards the heart,

heart, Akenſide obſerves, that they could not be called veins on this account, becauſe at this rate the pancreatic and biliary ducts might have been called veins alſo. Here however, he ſeems to have forgot, that the fluid in the pancreatic duct moves not towards the heart, but into the ductus communis, and thence into the duodenum. Akenſide has however invalidated the force of ſeveral of Monro's objections, particularly that of inconſiſtence, with which he is charged in admitting a communication between the blood-veſſels and lymphatics. He ſhews that he did not ſuppoſe that ſuch a communication between the arteries and the naſcent extremities of the lymphatics ſubſiſted, but at the places of their termination into the veins; the probability of which he confirms from experiments by Cowper. Upon the whole, though Akenſide plainly diſcovers, that he is offended with young Monro, yet he expreſſes it like a gentleman and a man of genius, and concludes with the following:

"And ſuch at laſt are thoſe ſlips, as Dr. Monro ſtiles them, which he is pleaſed to own may *perhaps* be thought *venial* in one who does not make anatomy his particular ſtudy. In return for which equitable conceſſion, he may (not *perhaps* but certainly) be aſſured, that Dr. Akenſide has ſo much partiality to a liberal ambition

bition in thoſe who are entering upon the world of letters and ſcience, that into whatever *ſlips*, or forward diſputes, or overweening concluſions, they may be drawn by it, in aſſerting their own pretenſions to any thing praiſe worthy; he ſhall think them all venial, *except want of candour:* nor would he have troubled himſelf, or any one elſe, with a word in anſwer to Dr. Monro's treatiſe, but for the paſſage quoted from it in the firſt of theſe notes."*

* Vide Monthly Review, October, 1758.

3. OF THE DISPUTE BETWIXT THE HUNTERS AND PERCIVAL POTT, ON THE DISCOVERY OF THE HERNIA CONGENITA.

THIS diſpute was originally began in conſequence of Percival Pott's publication of his diſcovery of that ſpecies of rupture, called *hernia congenita.* It appears, that at the time the enquiry into the various ſpecies of ruptures was conducting with zealous ardor by Percival Pott, the ſame had alſo occupied the attention of de Haller at Gottingen; and it alſo appears, that de Haller was the firſt pioneer in anatomy, who virtually and poſitively explored the true nature of the *hernia congenita:* that is, de Haller was the firſt of all anatomiſts, of all countries, who publiſhed the diſcovery; and it was de Haller who alſo gave it the name of *hernia congenita:* a name which defines the nature of the rupture; when the inteſtine in an Infant falls down into the *ſcrotum* after the *teſtis,* or along with it, producing thereby what he called *hernia congenita.*—Or according to the explanation of Pott, it is that particular kind of *hernia,* in which the portion of inteſtine or *omentum,* which occaſions the tumour, inſtead of being found alone in the hernial ſac, (as in a common rupture,) is found in contact with the naked teſticle; the bag containing

taining it being formed by the *tunica vaginalis teſtis.*

In the year 1755, Albertus de Haller publiſhed his account of the *hernia congenita*, and confirmed by it, that idea which had been previouſly floating from the imperfect obſervations of others; particularly from thoſe made by Sharpe, as remote as the year 1748. The remarks of Sharpe had called the attention of engliſh anatomiſts and ſurgeons to this object; and if ſuch a riſing character as Pott had not given the ſubject his cloſeſt attention, he certainly could not have been ſaid to be what he then promiſed, nor what he afterwards was acknowledged,—as a man by habit indefatigable, and endowed by genius, education, and honor, to merit the title in a ſuperlative degree, of being the moſt eminent ſurgeon of the age:—a man naturally ſo diſpoſed, as to be as far above the narrow ſolicitations for diſpute, as an expanſive mind could poſſibly elevate him.

De Haller's paper on the *hernia congenita*, appeared in the *Opuſcula Pathologica*, and was tranſlated into Engliſh in February 1756. The publication by Pott moſt undoubtedly appeared ſome time after, and it was at that immediate point of time when the diſpute commenced.

The Hunters took two grounds to make out their accuſation againſt Pott; one, that he had ſtolen the diſcovery from de Haller; another, that he had ſtolen it from them. They thought proper to make out two inditements againſt him, that if he ſhould be acquitted of one, there might be a chance, at leaſt, of his being convicted upon the other.

They firſt attacked him, for having ſtolen the diſcovery from them; but at the ſame time abuſed his performance,—abuſed that very performance which was ſaid to contain a valuable diſcovery belonging to them, and if not their's, was de Haller's;—abuſed that performance which, if (as they called it) ignorant, dull, or uſeleſs, was not an object for invidious contention;—not worthy to be claimed by them, from whom the diſcovery it contained was ſtolen,—nor to be defended by him who was accuſed of having committed the theft.

"The treatiſe," (ſays William Hunter), "came out in the month of February or March 1757. It aſtoniſhed me, if poſſible, more than profeſſor Monro's account of the lymphatics had done. It hardly contained one new idea. It was what any pupil of mine might have written; (for the caſes given at the end, ſupported only

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an uncontested fact) and yet neither my brother's name nor mine was mentioned. It bore strong marks of second hand observations, and of a time serving hurry in the composition. I complained of this at my lectures. Every person to whom I mentioned the subject, expressed his surprise; and the authors of the Critical Review made some reflections which could not be pleasing to Mr. Pott, and which one would have thought must have brought on some kind of justification."

Such was the style of the attack of the Hunters; and to say the truth, if Pott had not made a reply to it, he must have been more than a stoick; he must have confined the common passions of nature by something more than philosophy has suggested for binding them. Pott submitted to a defence; and so far as it is necessary to repel the dishonourable charge of his having stolen the discovery, by a previous conversation he had with John Hunter, I shall give an extract of his own words:

"I do not," (says P. Pott) "remember that the *congenial hernia* was once mentioned by either of us during my short visit, notwithstanding the Doctor has said that his brother shewed me his preparation with great readiness, and explained to me his (the Doctor's) hypothesis of the

the contiguity of the inteſtine and teſticle. Our converſation turned entirely on the paſſage of the *teſtes* from the belly into the *ſcrotum*; and, as far as I could perceive, (for he ſpoke with the moſt cautious, apprehenſive reſervedneſs) our ſentiments were alike.

" My papers were at this time finiſhed, and corrected for the preſs;——nor did I alter a ſingle ſyllable in them, in conſequence of this viſit to Mr. Hunter. But had that gentleman been half ſo explicit as his brother repreſents him to have been; had he been ſo ingenuous as to have told me, that either he or the Doctor had regarded themſelves as the diſcoverers; had he ſignified that either of them had any intention to ſay, or to publiſh any thing about it—I would either have ſuppreſſed my book, or have mentioned their names in it.—And as to the honour of the diſcovery, it would not have given me any concern at all.

" That is a ſhort and true account of the fact; this is the thing for which I have been traduced in print. The manner in which I attained my knowledge I have already moſt faithfully related. But excepting that ſingle circumſtance of not having related the ſhort converſation which paſſed between his brother and me, and from which I did

did not derive the leaſt degree of information. When I publiſhed my tract on the congenial rupture, I had no intention to anticipate either of them, or to prevent either of them from enjoying any reputation or honour, which might ariſe to them from their labours on this, or any other ſubject: if he (Dr. H.) had ſaid, that he or his brother was then enquiring into that part of the animal œconomy, I ſhould moſt probably never have proſecuted my enquiries,—as I ſhould have known that the ſubject was in ſo able hands: I want no reputation of that ſort."

This viſit was paid (by John Hunter's confeſſion) in the autumn of the year 1756; and Pott has proved an hiſtorical account of his diſcovery, by an appeal to names above the power of ſuſpicion. He ſays, that as the thing gave him much pleaſure, he procured a number of ſubjects, examined carefully, noted appearances, drew concluſions, made preparations, and ſhewed both the papers and preparations to his friends. And what the Hunters muſt think very remarkable, as they never had been uſed to any ſuch poſſibility, moſt of thoſe friends belonged to the *ſame Hoſpital*. Perhaps that was the reaſon why they could not bring themſelves to believe him. Among the reſt were Sainthill, Nourſe, Webb, and Hawkins. And when he had examined a great

great variety of ſubjects, he enlarged his notes, digeſted them into better order, and ſhewed them again to the ſame perſons.

Pott muſt have been a very bold man, if he had appealed to thoſe names without authority, and he muſt have been a longer time in bringing forth his diſcovery than John Hunter has given him, if this appeal of his be true: for his viſit to John Hunter was during the autumnal courſe which finiſhes with the year, and the treatiſe was publiſhed in February 1757 following.

There will never be an inſtance produced where any thing was obtained from the Hunters that referred to the improvement of ſcience, which they choſe to conceal; and where, from their luxury in imparting information, any advantage of them could have been poſſibly taken. The reaſon for their thus guarding all their proceedings, cannot eſcape him, who is furniſhed like me, with a clue directly leading to their hearts: who has acquired that maſterpiece over their motives as directly to account for all their undertakings, by diſcerning the intentions which induced them to ſet about them. This I ſhall corroborate by the following quotation.

" In

"In the latter end of the year 1755, when I firſt had the pleaſure of reading Baron Haller's obſervations on the *hernia congenita*, it ſtruck my imagination that the ſtate of the *teſtis* in the *fœtus* and its deſcent from the *abdomen* into the *ſcrotum* would explain ſeveral things concerning ruptures and the hydrocele, and particularly that obſervation which Mr. Sharpe had communicated to me, viz. that in ruptures the inteſtine is ſometimes in contact with the *teſtis*. I communicated my ideas upon this ſubject to my brother, and deſired that he would take every opportunity of learning exactly the ſtate of the *teſtis* before and after birth, and the ſtate of ruptures in children. We were both convinced that the examination of thoſe facts would anſwer our expectation, and both recollected having ſeen appearances in children that agreed with our ſuppoſition, but ſaw now that we had neglected making the proper uſe of them.

"In the courſe of the winter, my brother had ſeveral opportunities of diſſecting fœtuſes of different ages, and of making ſome drawings of the parts; and all his obſervations agreed with the ideas I had formed of the nature of ruptures, and of the origin of the *tunica vaginalis propria* in the *fœtus*. But till thoſe obſervations were repeated to his ſatisfaction, and were ſufficiently

aſcertained, he deſired me not to mention the opinion in my lecture; and therefore, when treating of the coats of the *teſtis*, and of the ſituation of the hernial ſac, &c. I only put in this temporary caution, that I was then ſpeaking of thoſe things as they are commonly in adult bodies, and not as they are in the *fœtus*: and at laſt, when I was concluding my lectures for that ſeaſon in the end of April, 1756, with a courſe of the chirugical operations, I gave a very general account of my brother's obſervations, and ſhewed both the drawing of Fig. II. which was then finiſhed, and the ſubject from which it was made."

I aſk whether it were poſſible to detect the proceedings of the Hunters, who had previouſly thus debated the matter of prudence; and who had, from that cauſe, kept back even what they knew (without de Haller, if any body believes it) from the very pupils who were attending them for information?—I aſk what the motive of the Hunters could have been, during the interval betwixt the publication of de Haller, which by this time had appeared, and that of Pott, which did not appear till the February following, for keeping back the important information, publiſhed by de Haller, and known to them, from their pupils? It was moſt aſſuredly to make themſelves appear, in the eyes of the pupils,

pils, *principals* in the diſcovery. For if they had avowed the diſcovery, without any illuſtration of it from their own diſſections, ſuſpicion would have directly given the credit to de Haller. It was prudent, therefore, in them to avoid ſaying any thing of the diſcovery, until they were prepared;—and during that interval Pott's untimely treatiſe abruptly appeared.

It is worthy elucidation in philoſophy, for the information of all men, from whomſoever the example may be drawn, to dwell upon this infallible axiom: that no one is ever ſo ſtrenuous for the reputation of genius, as he who has it not; and that no one is ever ſo careleſs of that gift of nature, as he who has it. Genius ſits eaſy upon him who intrinſically poſſeſſes it: he neither feels importance nor dignity from his own conception of its value, becauſe he cannot ſay, it belongs to him in any other than a relative ſenſe: he muſt derive his title to it, from the approbation of thoſe who alone can eſtimate the fruits of his invention. If ever there was an inſtance, where two men have been ſo often diſappointed by miſtaking themſelves, as the Hunters, I know not where to find it. All their diligence, their art, and their conteſts, only prove, that they ſtruggled indeed for it, but could never obtain

a reputation bearing the ſmalleſt reſemblance to men of genius.

Thinking they had ſecurely graſped the opportunity of riſing into fame, by making de Haller's diſcovery indirectly their own, they meditated their diſſections, when, miſerably unfortunate for them, theſe came too late, as in the mean time Pott's publication came forth. That was the ſhaft which ſtuck in their ſides:--that was the *lateri lethalis arundo* :—that was the cauſe of their abuſing Pott's publication, and—that was the ſimilar practice to which they had reſource, in the diſputes of injecting the *teſtis*, and of the abſorbent ſyſtem. If Pott's publication hardly contained one new idea, the Hunters could not have been injured: or if Pott confirmed the truth of his diſcovery, by a variety of *caſes*, which offered, from time to time, at the hoſpital he belonged to, and the Hunters did not, they could not poſſibly lay claim to his diſcovery; as without *caſes* there could not have been a diſeaſe of that nature known or deſcribed: nor could ſuch *caſes* have been brought forward dependant upon the will, but only when patients afforded the opportunity; and ſuch *caſes* being rare in their nature, there muſt have been ſome time, and a patient watching for collecting them. Theſe facts could never have been ſtolen from the

the Hunters, on Pott's viſit to them, within two months of his publiſhing; and the progreſs of theſe facts are atteſted by the ſurgeons already appealed to.

Or if Pott had taken up the idea from de Haller's publication, and confirmed it by theſe *caſes*; in that inſtance, the Hunters prove themſelves to have no other concern in the conteſt, than informing againſt Pott for ſomething taken from de Haller, and in that they prove too much for their own ſakes, as—"Whoever," (ſays Dr. Hunter) "will take the trouble of comparing the paſſages quoted from de Haller, and from Pott, will ſee that if Pott did ſteal at all, he ſtole the whole ſubſtance; and that no man could venture upon a more literal tranſlation with any chance of concealing the plagiariſm."

Pott, in his anſwer to this, declared that he had never ſeen, read, or heard of that work of de Haller, either in Latin or Engliſh, till twelve months at leaſt after his publication. This he avowed, as he emphatically ſays, to ſave the reader's time, and to cut ſhort the diſpute. But the part of his reply which was felt moſt ſeverely, from the truth it contained, was the following:

"Indeed

" Indeed the ſpirit of criticiſm, or more properly the deſire of finding fault, has, in this inſtance, got the better of that artful caution, with which Dr. Hunter moſt frequently either expreſſes or conceals his ſentiments, has carried him beyond the proper mark, and made him prove too much: ſince, if I had read the *Opuſcula Pathologica* of de Haller, previous to my publication of my general treatiſe in 1756, I muſt have obtained from thence, that very information which, the Doctor ſays, I got from his brother in 1757, at the ſame time when he is ſaid to have explained to me the Doctor's hypotheſis; for in that book, as I have already obſerved, are contained, both the doctor's hypotheſis (as he calls it) and John Hunter's diſcovery."

After the treatiſe of Pott on the *hernia congenita* had appeared, the conſternation in the lecture room of the Hunters muſt have been diſtreſſing to them; as in conſequence of the Hunters previous plan of prudence, for keeping back the diſcovery of de Haller, till they were prepared to make it their own by diſſections, the pupils were continued in ignorance of the whole of the diſcovery. And when that treatiſe, publiſhed by a ſurgeon belonging to one of the hoſpitals, and written in the vulgar tongue of their country,

country, could be read by the pupils, and the contents of it loudly whiſpered in the lecture-room, it was impoſſible but in the conſequence there muſt have been produced an exploſion: —it was too ripe, and too critical to be ſuppreſſed: and the Hunters were driven to ſay any thing againſt Pott, becauſe they could not ſay any thing for themſelves. It will hardly be credited, that, if their ſituation could if poſſible have been more diſtreſſing than it was, they would have confeſſed to the pupils, that they knew of the diſcovery, but had foreborn to demonſtrate it, from thoſe motives of prudence I have already explained.

I am now prepared to conſider, if I could, John Hunter's deſcription of the ſituation of the *teſtis* in the *fœtus*, with its deſcent into the *ſcrotum*. I ſhould have hoped to have conſidered the importance of this piece of anatomical performance relatively, at the immediate time it was in preparation, for being diſplayed in the lecture-room, either before his pupils or thoſe of his brother; but that privilege is denied me; as the firſt information we have of it is drawn from William Hunter's Commentaries. And I am very apt to conclude, that this performance took up more time than we know of; as John Hunter has given us to underſtand, in the ſecond page of

of his "Animal œconomy," that, "the following obſervations on this ſubject were taken from my notes, and publiſhed by Dr. Hunter in his Commentaries, to which I have added ſome practical remarks." I take it for granted, therefore, that theſe obſervations were never, or, if they were at all, not in any ſeaſon, read to the pupils during the diſpute: but I am more apt to conclude, that they were drawn up for the purpoſe of being publiſhed in the Commentaries, after the return of John Hunter from the wars. They never appeared before the public till March 1764, and it could not be avowed, that they were ever read at any time in the form they are publiſhed, as the rudiments of them, till the Commentaries came forth, were only in notes belonging to John Hunter.

This performance ſtands as the firſt ſubject in John Hunter's book on the "Animal œconomy;" and, independant of his experiments on the abſorption of veins, is to be conſidered as the firſt production of the author. As it is found in the Animal œconomy, it is very different from what may be ſeen in the brother's Commentaries; it has a new introduction, and contains much additional matter; there is a caſe annexed to it of the year 1767, only ten years after

ter Pott's caſes, which were ſaid to foreſtall them in the diſcovery.

Not being able, therefore, from any authority, to conſider theſe obſervations as relative to the diſcovery of the *hernia congenita*, I ſhall look upon them, as explanatory obſervations on a ſubject already diſcovered: and although they would have fairly been higher in merit, if they had not ariſen out of information already eſtabliſhed; yet in examining them abſtractedly, with the view of ſeeing what farther explanatory matter is offered upon a ſubject, which has been explained, they will be found to poſſeſs much independant merit: and I ſhould not follow the bent of my inclination, nor comply with the command of my underſtanding, if I did not voluntarily own, that John Hunter, by theſe obſervations, has fairly obtained anatomical fame.

One cannot therefore refrain from lamenting more ſeriouſly, that they ſhould have been produced in a turbulent ſtorm of contention, in a paroxyſm of envy, and in an act of claiming profeſſional rights belonging to others. I ſhould have received more infinite ſatisfaction, if John Hunter had given me the opportunity of applying to him, the lines which Denham wrote on Fletcher,

Fletcher, and closing these disputes more honourably to his fame, by the following.

——————— I need not raise
Trophies to thee from other men's dispraise;
Nor is thy fame on lesser ruins built,
Nor need thy juster title the foul guilt
Of eastern kings, who to secure their reign
Must have their brothers, sons, and kindred slain.

I find myself now approaching to that page of the life of John Hunter, which cannot afford me pleasure, if it were not derived from a conscious intention of supporting truth: and as I know from what will be told by me, that the credulity of his admirers will be first shocked, and from the natural transitions of the human passions, their anger will be next rouzed; it is for that reason, and that alone, I have determined to be as explicit as possible.

John Hunter has published on many subjects; and if the eye of criticism were to peruse the whole of them for this particular purpose, I do not know but the opinion would be, that the explanation, the language, and the style of his writing were at least ample to the expression of his ideas: and that, if there be any obscurities

ſcurities and any errors, as there moſt certainly are in very great abundance, theſe do not ſo much ariſe out of defect of language and ſtyle in his writing, as from a native obſcurity in his ideas: they are moſt commonly, if not always, the conſequence of a confuſion in his mind.

If I were not to enquire into the truth of this queſtion, however delicate it might appear to thoſe who wiſh that it ſhould not be enquired into,—that truth which is poſitively neceſſary to be made known, and which preſſes hard for the cleareſt explanation, would hereafter be doubted; although I am now juſtified in ſaying, that it cannot be contradicted. Poſterity might otherwiſe have ſaid from perſuaſion, eſpecially from what appears upon the face of his publications, that John Hunter was a man of conſiderable knowledge in literature;—and poſterity would prove it, in the plaineſt manner, by referring to the papers of experiments and obſervations now before me,—to the many papers publiſhed in the Philoſophical Tranſactions of the Royal Society,—and to all his other works.

The truth is, that he only furniſhed the images, and that the writing part was always performed by another:—he prepared the ſkeleton, and another covered it with compoſition:—he

found the materials, and another made them up into dreſſes for the public :—he was incapable of putting ſix lines together grammatically into Engliſh; and, at his lectures,—he was often found ſo far incapable of making out the ſenſe of his own notes, as to paſs over the ſubject they were meant to explain.

It was owing to want of education, that his notions of things were ſo very imperfect, and his conceptions ſo very contracted: inſtances ariſing from this original defect, are to be found throughout his images, and if they had been confined to them alone, they might have paſſed without obſervation: but they operated ſtrongly in his conduct towards others; and not only the profeſſion, but thoſe who follow it, have experienced in a very unpleaſant degree his vulgarity from a want of the poliſh of education, as will be made hereafter apparent.

The three diſputes which have paſſed already before me, were carried on in a monthly publication, called the Critical Review: and the ſyſtem of that undertaking was in a great meaſure broken in upon, to be made ſubſervient to the purpoſes of the Hunters, who had obtained that aſcendancy over the Editor, as to command his

his ſervices for the promotion of any cauſe, which their views or ambition ſuggeſted.

It is neceſſary for me, from the motive of truth therefore to ſay,—that all the attacks, and all the replies of the Hunters, which gained ſo willing an admittance into the Critical Review, during theſe diſputes which have engaged my attention, were reviſed, corrected, and publiſhed under the immediate eye, influence and direction of Smollet——Smollet—the author of the Hiſtory of England, Humphry Clinker, Peregrine Pickle, Roderick Random, and if I miſtake not, the Tars of Old England,—and Smollet—the then editor, and, if I miſtake not, one of the proprietors of the ſame Critical Review. Smollet was at their command for any ſervices they wanted of him;—for whatever was calculated to raiſe their reputations, by the ruin of thoſe who ſtood in their way, his praiſe and his abuſe were equally at their obedience: And to ſay the truth they could not have ſelected, out of every circle of authorſhip upon the face of the earth, a more bitter or clever fellow, not only for conſolidating their ideas, but alſo for conducting them forth to the public.

I only attribute the keen conduct of the diſputes on the part of the Hunters, as they are to

be

be found in the Critical Review, to the diſintereſted aſſiſtance of Smollet. I do not mean to be underſtood in ſaying that Smollet extended his ſervices farther. So far as I have introduced Smollet, was purely to explain the zeal of that Review in the cauſe of the Hunters. This epiſodical digreſſion does not impair in the leaſt the force of my general aſſertion,—that John Hunter never was the author of any production which has appeared under his name. But if I had not thus previouſly grounded my aſſertion,—if this fact had not been known, his admirers might have been aſtoniſhed at my imputation of his want of literature; and more eſpecially, when in the two productions I have already examined, they read the Greek anatomical derivatives, moſt accurately given;—and the Latin terms in anatomy moſt nicely correct;—ſo as there cannot even be found, what careleſſneſs in ſcience will frequently commit,—the mark of a *caret* (∧) omitted. If I had not thus explained myſelf, I ſhould have been told, that John Hunter's experiments on the abſorption by veins, and his obſervations on the ſtate of the *teſtis*, appeared not at the time when the diſputes in the Review were carrying on, but ſo long after as in the year 1764. I acknowledge the fact, and by it mean to ſay, that I do not know, that Smollet wrote

wrote *them* for him, but I know that John Hunter could not have written them.

If I had tacitly acknowledged, that the diſputes in the Critical Review, and the two publications under the name of John Hunter, had been written and conducted by himſelf, I ſhould have proved too much for the belief of his admirers.—And as the ſting of the critic, throughout the whole of the diſputes, was acutely pointed, —as the argument was conducted with a poignancy that cannot be outdone,—charged with Greek and Latin quotations, which the lore of literature alone can furniſh,—it might have been concluded, that John Hunter had once been ſenior wrangler at one of our univerſities, and that it was fabulous altogether that he ever came from the workſhop of a carpenter directly into the diſſecting room of an anatomiſt. Now this his admirers will hardly venture to admit, or that he ever pleaded his inexperience in literature, as an excuſe for his errors,—or for his deriving aſſiſtance from others. If ever in all his publications, or in whatever his admirers have ſaid of him, an apology for his imperfections had been offered, *that* might perhaps have ſilenced the obſervations of his opponents, or at leaſt have ſoftened their criticiſms: but he always conducted himſelf, as if he really had fed on

on the choiceſt fruit of ſcience, which the tree of learning can only produce.

I ſhall for a while turn away my attention, from the vain flights of him, who thus attempted to ſoar, like another Icarus, with dedalion wings, to contemplate the character of the immortal de Haller, and to ſhew a contraſt that in itſelf will be fully deſcriptive of my intended motive.

In the three ſeparate diſputes which I have demonſtrated, de Haller's ſhare in the diſcoveries has been already diſtinguiſhed: and curioſity would naturally preſuppoſe, that thoſe contentions poſitively aroſe out of the importance, and value of the three diſtinct anatomical rights;—that the juſt title to the diſcoveries would have equally awakened a jealouſy for anatomical property in every one of the parties. But theſe objects were never ſeen by the eye of this philoſopher in any ſuch magnitude. De Haller purſued his anatomical ſtudies without conſulting his breaſt upon the impulſe of fame, or without feeling ſuffocations of jealouſy. What he was diſcovering was the reſult of his natural taſte; and what he had diſcovered was conſidered by him, as dead game by the true ſportſman; with a liberality of heart he gave it away, and went directly in purſuit of ſome other inveſtigation.

The

The following abſtract has been made from the ingenious Henry's life of him.

De Haller was born in the year 1708. In his early infancy he manifeſted an activity of mind, a faculty for labour, and a ſtrength of memory neceſſary for thoſe who are deſirous of comprehending many ſciences and purſuing great operations. He was the firſt ſcholar of his time; and in his juvenal days delighted in poetical effuſions, of which he has exhibited many valuable ſpecimens. But he ſoon gave up the purſuit of poetry for the inveſtigation of nature; and made choice of the only profeſſion which would allow him to devote himſelf to that ſtudy without reſerve, namely that of phyſick.

On entering upon this intenſe undertaking, he renounced wine for ever, that he might be certain to avoid the abuſe of it; and in order to guard more infallibly from ſeduction, he thought himſelf obliged to obſerve a rigorous ſeverity in his manners. He began his ſtudies at Leyden, where he found an anatomical theatre well ſupplied with ſubjects; cabinets of natural hiſtory; a very extenſive library, and every thing which could encourage and invite to ſtudy. There he found himſelf in company with Boerhaave, Albinus then young, and the famous Ruyſch,

the great improver of anatomical injections and preparations, ſtill living at Amſterdam, proſecuting his ſtudies at the age of ninety. Here he took his degree; the theſis for which was on the ſalivary ducts, in which he diſplayed the knowledge he had acquired in anatomy, and proclaimed himſelf an obſerver capable of enriching that ſcience with many important improvements.

In 1727 He viſited England; was introduced to Sir Hans Sloane, who had at that time formed his collection of natural curioſities, and had the pleaſure of becoming acquainted with Plumtree, Cheſelden, and Douglas, men diſtinguiſhed throughout Europe for their profeſſional abilities. From England he went to France, and was in danger of proſecution for obtaining dead bodies. He then went to Baſil and ſtudied botany; and returned to his own country in the year 1730.

The firſt poetical productions of de Haller were by him committed to the flames. His taſte for poetry now returning, he bridled it in ſuch a manner that its charms ſhould not be ſufficiently alluring to detach him from the more ſevere and uſeful ſtudies. He only cultivated the muſes in his ſolitary walks, in thoſe hours of the night when ſleep forſook him, and during thoſe receſſes

receſſes from labour with which his ſtate of health ſometimes forced him to comply.

His immenſe labour in anatomy employed the largeſt ſhare of his time; and tho' ſeparated from his maſters, friends and competitors, his own private cabinet and ſelect library ſupplied the place of academic aid. Here he laid the foundation of that vaſt extent of knowledge which comprehended every ſpecies of literature. The diſcoveries of every cultivated age and nation were extracted in the courſe of his reading, which he continued with unremitting attention during his whole life; without being diverted from it by the viciſſitudes of fortune or embarraſſments of affairs.

In 1736 he made botanical excurſions, aſcended the mountains of Jura and Alps, and deſcended to the marſhes in Switzerland. The ſtudies of mineralogy and zoology were equally extenſive to his comprehenſion. The republic of Berne eſtabliſhed for him an amphitheatre where he taught anatomy.

Soon after he was invited by George II. to promote the univerſity of Gottingen; and there was eſtabliſhed for him an anatomical, botanical, and ſurgical profeſſorſhip. This he accepted,

accompanied by a young wife, whoſe perſonal qualities had captivated his heart, who had borne him children, and who, by the ſweetneſs of manners with which ſhe had adopted his taſte and purſuits, formed the happineſs of his life. But this undertaking proved fatal to his dear Mariamne, who died in conſequence of an accident which befel her on the journey.

The regency of Hanover gave him every proof of their eſteem for talents; and it was thus that he eſtabliſhed that fame of Gottingen, ſo juſtly to this day celebrated over the world. He was ſo truly original in phyſiology, that he might be fairly ſaid to have been the parent of it. To this end he inveſtigated the ſtudy on exact anatomy of man and other animals. Nor was it till after thirty years of labour, that he thought himſelf juſtified in publiſhing his diſcoveries, and which was the æra of a revolution in anatomy principally owing to the powers of de Haller.

A review of new publications was undertaken by him in the whole circle of medical ſcience, in natural hiſtory, phyſics, chemiſtry, metallurgy, and œconomics. He undertook the review of the different articles, beſides hiſtories, voyages, and deſcriptions of climates and ſoils. By the influence

influence he had with the princes of the empire, he formed the undertaking of Mylius to travel through America; and by the intereſt of George II. de Haller was made a baron of the empire. After an abſence of 17 years, he returned home to Berne, where he was elected a member of the ſovereign council: a title which enabled him to fill ſeveral places in government, and to one he ſucceeded by lot in 1753, when the adminiſtration of the ſalt works was brought by him to perfection.

His attention to the duties of a magiſtrate did not entirely take him off from his phyſical purſuits. His experiments on incubated eggs were made at Berne. He compleated his phyſiology, arranged his library, and collected his works. He furniſhed the Supplement of the Encyclopædia with articles on the ſubjects of anatomy, medicine, and phyſiology. As perpetual preſident of the univerſity of Gottingen, he remitted not, during his abſence, his attention to its intereſt. He was offered by George II. the chancellorſhip of it: this produced a conflict in his mind, whether he ſhould leave his native country the ſecond time for Gottingen. But it was determined that he ſhould remain at Berne: that republic, deſirous of retaining and fixing him more firmly to the ſervice of the ſtate, aſſured him

him of their wiſh, and ſettled on him a penſion for life.

He died in the year 1777: a memorable year for the loſs of the modern age; in which departed beſides de Haller, Voltaire, Linnæus, and Rouſſeau. His valuable library, conſiſting of 13512 volumes, on the ſubjects of anatomy, ſurgery, the practice of phyſic, botany, and natural hiſtory including his diaries, *herbaria viva*, and about 150 manuſcripts, moſtly written in his own hand, was offered to the London bookſellers, a number of whom agreed to unite in treating for it; but before they had taken any farther meaſures, the whole was purchaſed by the emperor of Germany.

De Haller was moſt agreeable in converſation. His elocution was free, ſtrong, and conciſe. His immenſe reading, fertile and faithful memory, and ſound judgement, enabled him to adapt himſelf to all diſpoſitions. In his perſon he was tall and finely proportioned. His countenance, which had a ſerious caſt, from being ſhort ſighted was full of expreſſion. He was ſuperior to the affectation of wit, and diſdained to make a parade of the knowledge he poſſeſſed. His ſoul was gentle and his heart replete with ſenſibility.

PART

PART II.

FROM HIS ENTRANCE INTO THE ARMY, WITH CONSEQUENT TRANSACTIONS, TO 1770.

THE ſtudy of anatomy is generally purſued from three diſtinct motives.—

1. To lay the neceſſary foundation for thoſe who are hereafter to follow the practice of phyſic and ſurgery.

2. To enable thoſe, who purſue it with more than common ardor, and with a longer attention to the ſubject, to obtain the qualifications,—not only by inveſtigating the human ſubject but alſo the various claſſes of animals,—neceſſary for becoming hereafter teachers of the art of anatomy.

3. To furniſh the philoſopher of independant fortune, whoſe delight it may be to inveſtigate the operations of nature, with the only true means of arriving to any degree of perfection for the accompliſhment of that end.

The

The firſt and ſecond are the general motives, the third is particular and very rare indeed!

Of the firſt,—the motive is founded upon neceſſity, as without the knowledge of anatomy, the practice of phyſic and ſurgery would be diſgraceful to thoſe who purſue it, and detrimental to ſociety who ſought for relief from ignorance: the ſtudy of it therefore from this motive brings annually to London nearly three hundred pupils, who diſtribute themſelves at the various hoſpitals, and when the hoſpital hours for ſeeing patients are over, fill up the remainder of the day by attending the courſes of their favourite anatomical lecturers. Two courſes are uſually read in the year; the firſt commencing in October and ending in December, the ſecond in January and cloſing in April.

Of the ſecond,—the motive ariſes from a laudable propenſity; as the lecturers in anatomy are characters of the higheſt importance to ſociety, and ought to be regarded with a degree of veneration. For acquiring their knowledge, they muſt overcome toilſome difficulties, and eſcape the dangerous effects of unwholeſome proceſſes. Their intercourſes with intellectual nature cannot be obtained but by ſevere attention, and under particular inconveniencies: they muſt purſue

ſue their enquiries in the coldeſt ſeaſon, and from want of a better regulation, are reduced to keep up a connection with a neceſſary order of men for obtaining the materials, without which they could not, in due ſucceſſion of ſeaſons, diſplay the reſult of their laborious and ingenious diſſections to their pupils.

This valuable branch of ſcience, which diffuſes its influence into the practice of phyſic and ſurgery throughout the dominion, and from which every ſubject of it ſooner or later receives his portion of good effect, is conducted by rarely more than four or five lecturers in London. Within theſe ten years, five are now freſh upon my memory, who have fallen the victims of putrid *myaſma* in the prime of life, whilſt they were fulfilling this important duty for the welfare of their fellow beings. The expences for procuring ſubjects and large rooms are very conſiderable; and it might have been hoped, that an undertaking ſo dangerous to health, ſo unpleaſant in the proceſs, ſo laborious in acquiring, and ſo hazardous in fortune, would procure in return an adequate emolument. But I have reaſon to believe, that the profits among all the anatomical teachers amount altogether to no more annually than three thouſand pounds.

From the account which is given by William Hunter of his brother John in the commentaries, John had, during his diſſections for his brother, experienced ſome ſevere indiſpoſitions, which retarded his progreſs in anatomical engagements: and this might have been one cauſe why he renounced the lecture-room, or at leaſt relaxed ſomewhat of that ſevere application, which was demanded from him who was hereafter to become, what his brother then was, an anatomical teacher of ſome eminence. Perhaps the cauſe of ill health, together with his want of education, and without knowledge of the technical terms of the art, or ability to deliver, either orally or by writing, thoſe neceſſary *formulæ* found in preſcriptions, deterred him, at that time, from purſuing anatomy with any intent of becoming hereafter a regular teacher. There muſt have been ſome ſuch reaſon for his abruptly declining this purſuit for the army: as he who was wanted to aſſiſt his brother before, and when the fame of his lectures was annually increaſing, muſt equally be deſirable when that fame had crowded the theatre: moreover than all this, in the abſence of John Hunter, another ſupplied his place, which proves at leaſt that he was not then ſuited to the undertaking.

This

This was the commencement of his being a ſurgeon. And in May 1756 he became the houſe-ſurgeon to St. George's Hoſpital, in which ſituation he only continued for about five months: he reſigned the office in September following. He ſucceeded to Hopkins, and was ſucceeded by Gunning, the preſent worthy ſurgeon-general to the army.

Upon the ſtricteſt enquiry, I have reaſon to be perſuaded that this was all the ſurgical education John Hunter ever received: and to my own knowledge I can ſpeak it, that the period of five months duration at the hoſpital in the office of houſe-ſurgeon is the ſhorteſt which can be found in the unerring journals of hoſpitals. The uſual time for the reſidence of a houſe-ſurgeon is generally twelve months, but ſometimes it has been extended to two years: and the uſual mode of ſelecting the houſe-ſurgeon to that office is, by his firſt having as a dreſſer diſcovered ſufficient ſigns of diſpoſition and attention to duty, as to merit the preference: for the office of houſe-ſurgeon is of great importance to the intereſts of the hoſpital: his duty conſiſts in receiving and attending to all accidents, in ſending to the ſurgeon upon an emergent occaſion, in attending to the preſſing calls of the ſurgical patients during the abſence of the ſurgeons, in ſuperintend-

ing the conduct of the pupils, in keeping the inſtruments under his care in order, in furniſhing the preparatory dreſſings on days of operations, and in holding the key of the dead houſe.

John Hunter's education ſeems to have been upon an inverted ratio to all other ſurgeons. He to become a ſurgeon, ſerved a long apprenticeſhip to anatomical purſuits, and only five months to ſurgical: whilſt others, to become ſurgeons, ſerve their apprenticeſhips with ſurgeons; and for a year or two purſue their anatomical ſtudies, and that at a period of life too, when their minds are in preparation, and their ages favour the reception of that important acquiſition to practice.

Anatomical information is purely a mechanical ſtudy: whereas the art and practice of ſurgery conſiſt in a general knowledge of eſtabliſhed principles, and a deſire for excellence reſulting from obſervation: the mind is conſtantly in the exerciſe of improvement, and practice preſents frequently a caſe that demands a freſh exerciſe of the judgment. Beſides the requiſite aid of anatomy, no one can be ſaid to be a perfect ſurgeon, without having ſpent ſome time in obſerving the practice of the *materia medica*. His ſhort reſidence at the hoſpital would not allow John

John Hunter to impreſs upon his mind the general outlines of ſurgery, and for want of which his taſte for ever after appeared to be vitiated; and his being totally unacquainted with the *materia medica*, rendered all his preſcriptions bald and informal.

But there is one ſaying by John Hunter which appears to be ſo very paradoxical, that from his admirers alone the explanation of it muſt come. He conſtantly aſſerted that he never read:—was it then poſſible for any man poſſeſſing, as he did, but barely the mechanical acquirements of anatomy, without education, without reading, with five months application to ſurgery only, and without knowing any thing about the *materia medica*, to eſtabliſh the true excellence of the art, founded upon general principles, of a ſurgeon? But notwithſtanding what he has been known to aſſert to the contrary, I believe that he did read; and I believe he was ſo much in admiration with the idea of being ſuppoſed to make rules in ſurgery for himſelf, that he hoped, by thus denying that he read, to avoid being detected, when he borrowed from another whatever was his deſign to be paſſed for his own.

Without there had been a fixed intention of his becoming hereafter a teacher in anatomy, it might

might be certainly conſidered as ſomething too much for John Hunter to continue on the purſuit; as the rewards of the art are only derivative either from acquiring the requiſite knowledge for a teacher, or from aſſiſting the education of a ſurgeon.

He therefore, to lay a foundation for becoming a practical ſurgeon, obtained an appointment, I believe, upon the ſtaff in the army; and in the year 1761 was with the army that took Belliſle; and in the ſubſequent year, he accompanied the army to Portugal, returning to England in May.

The *Memorabilia* of a great man are ever of ſome account: and therefore, although it be not the leading province which I have aſſumed, I ſhall note a trifle or two of thoſe occurrences, which have been handed down with unerring fidelity during his abſence. It was at Belliſle that he firſt took to cramming the ſtomachs of lizards and worms, and firſt *diſcovered* a never failing method of diſſolving every bubo without its coming to abſceſs. As I have treated this diſcovery after the manner it merits, in my obſervations on his treatiſe on the venereal diſeaſe, I ſhall ſpare any farther critical remark.—And it was at Portugal, that he made an experiment which

which, as he has deſcribed it, was for aſcertaining the faculty of hearing in fiſhes.

Theſe valuable portions of anecdote are drawn from what John Hunter has ſaid of himſelf. But I think I need not have been ſo explicit upon the *diſcovery*, as no one upon the face of the earth would have ever ventured, or ever will, to aſſert for him, that he could do what he there ſays he did:—that he could diſſolve every bubo without its coming to abſceſs, no one but John Hunter himſelf would dare to ſay it:—but what is very unfortunate after all, no one who knows any thing of the matter ever believed him, or did he believe it himſelf; for in his book on the venereal diſeaſe, there are eight inſtances of buboes coming to abceſs under his own care.

Of the diſputes which have taken up a great deal of my conſideration, it might have been preſumed, that John Hunter was rather a partaker than a principal; and from that, an inference might have been drawn, that when in a ſtate of ſeparation from his brother William and anatomical points, he would be found to ſtand thus alone, without his diſcovering that turbulence of temper which, as hath ſince appeared, he natively and radically poſſeſſed.

He

He had ſcarcely arrived at Portugal, before he excited an uneaſineſs among the faculty, which their ſituations had never experienced before. He turned the common intercourſes of ſocial good humour into ſuſpicious tauntings of jealouſy: He created a faction and a conſequent diſguſt. This brought on an explanation from one who was his ſenior in the army by ten years;—from one who had been a faithful follower of the fortune of the Duke of Cumberland, and had dreſſed his wounds in battle: He was rouzed to draw his ſword upon John Hunter, which was ſheathed without the quarrel being reconciled—for what reconciliation can remove ſuſpicion!

The confirmation of this I am not diſpoſed to doubt, but ſome there are who may: thoſe I will aſſiſt as far as it is in my power by aſſuring them, —that the manly veteran Tomkins, of Park-Place, is very capable of explaining the fact.

On his return to England, and at the cloſe of the war, he took a houſe in Golden-Square, and found himſelf in point of fortune, better than nothing by his half pay; that enabled him to pay his houſe rent, and ſome other neceſſaries, requiſite ever for thoſe who ſit down in practice waiting for patients:—and here commences his firſt career of a London ſurgeon.

What

What happened to John Hunter, happens to every ſurgeon in the beginning: there was not employment enough furniſhed by the practical art, to fill up the active hours of the day. But his reſources were in an extraordinary degree advantageous over moſt young ſurgeons under the ſame predicament for want of patients: he experienced no laſſitude: he had furniſhed his mind with the means of employment, and to ſay the truth, he was of an uncommon turn to induſtry.

He opened a room for diſſections, and demonſtrated ſubjects to his pupils: he began to make preparations upon his own account: he carried on thoſe reflections with a new vigour, that had palled upon his mind before he went into the army, and he, or ſome one elſe for him, put into a condition thoſe papers on *the Injection of the teſtis*, on the *Abſorption by Veins*, and on *the State of the Teſtis in the Fœtus*,—and on *the Hernia Congenita*, which were not publiſhed until the year 1764, as I before have declared.

He had not at this time exacted thoſe rigid ſeverities of temperance to which he was obſerved to adhere at his latter part of life. John Hunter at this time, and for ſome time after,

was a companionable man: he aſſociated in company, drank his bottle, told his ſtory, and laughed with others. And in all probability by that ſuppreſſion of the ſolitary paſſions, which never could have befriended his eaſe,—by ſuſpending their practice and their miſchief, he found himſelf an extraordinarily happy man. But theſe halcyon days did not laſt long. He found alſo what is commonly true, that familiarity did not enhance profeſſional eſtimation. He was ſoon ſickened of the diſſipation he had ſomewhat contracted by the ſociety of officers from the army, and more readily yielded to his natural inclination for indulging his calls to ſpeculative points of natural hiſtory and comparative anatomy.

In February 1767 he was elected a fellow of the Royal Society; and in December 1768 he was choſen ſurgeon to St. George's hoſpital in the room of Gataker.

In 1770 he removed from Golden-ſquare to Jermyn-ſtreet, and took the houſe which his brother had left when his own was finiſhed which he had built in Windmill-ſtreet.

During all this time he found himſelf at leiſure for meditating plans of life, that plainly denote an

an intrepidity of mind and a vigour of application, which natural obſtacles inſurmountable by moſt, could not ſuppreſs in him; which few have ever attempted, although ſupplied by the compleateſt aid of early academic learning.

The practice of ſurgery had at this time, or for long after, afforded no opening for him. Hawkins, Bromfield, Sharpe, and Pott, embraced almoſt the whole of family practice; whilſt Adair and Tomkins carried from him the chief of the practice derived from connections formed in the army.

His ſole dependance was on his anatomical power; and from that he drew the greateſt advantage it could poſſibly admit. He had the art of giving a conſequence to every thing that he did, by the dextrous uſe he made of his anatomical machinery. He aimed to be admired rather than uſeful—marvellous than true—difficult than important. Of this I ſpeak from a thorough conviction, that if John Hunter had experienced a better education, he never would have gone the length which he did, nor ſucceeded ſo highly.

With the exception of what was publiſhed in his name by his brother William, in the year 1764, there does not appear to be any thing by

John on his return from the war, up to the year 1772. If there were any publications, they muſt have terminated like many more by others, they muſt have experienced the fate of abortions, or at leaſt I know nothing about them. And here was an opportunity of ten years, which a man of true genius would have embraced. He would, though late in life, have laid that foundation of literature, which had eſcaped him in his earlier part; or he would have declined the vanity of public fame, for the private cultivation of uſeful knowledge. John Hunter did neither the one nor the other.

His profeſſed delight was the ſtudy of the animal œconomy: but his ambition carried him far beyond the uſeful proſecution of that ſcience, ſo flattering to the laudable diſpoſition of thoſe, who purſue it purely for information. He was never diſcovered in attempting to explore the occurrences moſt neceſſary to be accounted for, as the pride of his heart was only to ſelect an obſcure ſubject, which involved in it ſo much matter of wonder as to raiſe the public attention, or to make that his own which another had juſt taken up before him.

He cared not about the truth, nor the uſe which might be made of any inveſtigation in nature;

nature: and if he could give his ſubject the air of novelty, he cared neither from whom he took his information upon it: both his repute for underſtanding, and his hope for ſucceſs, depended upon his paſſing the idea for his own.

Objects out of the ſight, and beyond the reach of common obſervation,—objects in their nature ſingular, inſignificant, and uſeleſs, were chiefly among his moſt favoured ſelections. From the year 1772 to the year of his death, he publiſhed a paper of ſuch a deſcription. Every revolving ſeaſon came forward with a freſh ſupply of his bounty. He aſſumed the high office of Nature's ſtorekeeper. He ramified, dilated, and expanded her hidden ſecrets,—to the prying eye of common curioſity,—or rather to the uninformed admiration of vulgar credulity. I aim not to confine this ſentiment to any particular rank which men hold in ſociety, and which is the mere creature of fortune—not to meaſure it by a ſcale of perſonal or hereditary property, but to extend it to the condition of the mind as reflected by actions; and then it will be ſeen to a demonſtration, that the credulous vulgar may be equally tenants in ſquares and in alleys.

He had the wonderful art of hanging heavy weights to ſlender wires; and by this he contrived

trived to have his papers preſented to the Royal Society, and to obtain a reading of them. But leſt the Philoſophical Tranſactions, which are difficult to be got at, ſhould not ſerve to promulgate what he had written, he has collated the papers, and publiſhed them, with other matter, under the title of "*Animal œconomy.*"

An obſervation frequently illudes the due impreſſion, when for the evident truth it contains one is obliged for the information upon it to another. With this regard I refer every one who peruſes the productions of John Hunter. Whenever he undertook to treat upon a ſubject already treated upon by another, his aim was either to paſs the author over in ſilence, or in his way, to foreſtall the invention, by alluding to notes remotely ſaid to be made upon the ſubject by himſelf, and excuſing himſelf for not having publiſhed them before. Such has been his habit from the beginning of the diſputes I have already conſidered; and it will too plainly appear, that he never was aſhamed of the vice—that he never endeavoured to correct it, and—that it never forſook him.

If the immortal Newton had been his cotemporary, theſe notes would have been called forth

as

as vouchers, for contending with him the right in diſcoveries.

This is a catalogue of his papers, publiſhed in the *Philoſophical Tranſactions*, with their dates.

	vol.	*page.*
June 18, 1772. On the Digeſtion of the Stomach after Death. - -	62	447
July 1, 1773. Obſervations on the Torpedo.	63	481
February 27, 1774. Of certain Receptacles of Air in Birds. - -	64	205
March 17, 1774. On the Gillaroo Trout.	64	310
May 11, 1775. On the Gymnotus.	65	395
June 24, 1775. Experiments on Animals and Vegetables, with reſpect to their Power of producing Heat. -	65	446
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February 25, 1779. Account of the Free Martin. - -	69	279
January 17, 1780. Account of a Woman who had the Small Pox during Pregnancy.	70	128
June 1, 1780. Account of an extraordinary Pheaſant. - -	70	527
November 14, 1782. Account of the Organ of Hearing in Fiſhes. -	72	379
March 7, 1785. Anatomical Remarks on a new Marine Animal. -	75	333
March 22, 1787. An Experiment to determine the Effect of extirpating one Ovarium upon the Number of Young produced.	77	233

April

	vol.	page.
April 26, 1787, Obſervations tending to ſhew that the Wolf, Jackall, and Dog, are of the ſame Species. -	77	253
June 28, 1787. Obſervations on the Structure and Oeconomy of Whales.	77	371
April 30, 1789. Supplementary Letter on the Identity of the Species of the Dog, Wolf, and Jackall. -	79	160
February 23, 1792. Obſervations on Bees.	82	128

Six Krohnian Lectures on Muſcular Motion, from 1776 to 1782.

I have given an account of thoſe papers, which were accepted by the Royal Society: and I have more to add of a ſimilar deſcription, which were not offered, or if offered, were not accepted.

Obſervations on the glands ſituated between the rectum and bladder, called Veſiculæ Seminales. -	*Animal Oeconomy.*
Of the Structure of the Placenta.	*idem,*
Some Obſervations on Digeſtion.	*idem.*
On a Secretion in the Crop of breeding Pigeons for the Nouriſhment of their Young.	*idem.*
On the Colour of the Pigmentum of the Eye in different Animals. - -	*idem.*
The Uſe of the Oblique Muſcles.	*idem.*
A Deſcription of the Nerves which ſupply the Organ of Smelling. -	*idem.*

Theſe

Theſe might be fairly termed pleaſurable amuſements reſulting from the intenſe pains of a ſtudent of nature: but to John Hunter they brought an eſtimation ſuperior to any improvement in the art of ſurgery: he never made uſe of that art, but for the emolument he derived from it: he conſidered ſolely the profits of ſurgery, as a means for carrying on the expences of ſpeculation. If he had been a man of independant fortune, he would have been a naturaliſt: and as he was a ſurgeon without fortune, he made that profeſſion ſubſervient to his favorite paſſion.

The following are his chirurgical productions:

1. The Natural Hiſtory of the Teeth, in two Parts, containing 258 Pages, 4to. with Plates. Price 1l. 1s. 1778.

2. A Treatiſe on the Venereal Diſeaſe, containing 398 Pages, 4to. with Plates. Price 1l. 1s. 1786.

3. Obſervations on the Inflammation of the Internal Coats of Veins.—A Paper publiſhed in a Volume of Tranſactions for the Improvement of Medical and Chirurgical Knowledge. 1793.

Theſe three are the only performances claimed by him within my knowledge; and I certainly ſhall not hunt about for others; as, if there were any more by him belonging to the art of ſurgery,

I ſhould imagine they would,—by ſounding their own importance, ſpeak for themſelves.

I ſhall proceed to make any curſory remarks which might offer on my peruſal of thoſe abſtract portions of obſervations upon the animal œconomy, without dwelling with much obſtinate tenacity of criticiſm upon them. Errors or ſingular opinions, which might be found among theſe, are as unimportant to the immediate and neceſſary purpoſes of man, as a miſconception of the ſimpleſt product in the creation can poſſibly be:—whereas the works of John Hunter, which preſcribe rules for the practice of ſurgery, refer to the moſt direct and neareſt intereſt of ſociety. Errors in ſurgery, once eſtabliſhed by the authority of a great name, become miſchiefs to which imagination can trace no boundary: they might be ramified to every diſtrict of the world, wherever the implicit pupil who has imbibed them reſides;—and when he has ſucked in the poiſonous notions directly from him who has created and diſpenſed them, the conſequent effects will be ſtrongly experienced by the public at large.

It is much more common for man to follow the inſtructions of thoſe whoſe abilities he eſteems, than to permit his underſtanding to act from

from the reſult of its own reflection. Human talent will rarely allow of few who can correct opinions, which have been formed in the early part of education, by the after exerciſe of their own judgment.—And few, from among many, who follow with exactneſs the inſtructions they learnt in their youth, can boaſt of the active ability of amending or correcting prejudices become habitual from their early impreſſions.

I ſhall take the ſubjects according to their dates; and ſhall, by conſidering, in their turns, natural, anatomical, and ſurgical productions, procure that change upon the mind by which attention is more likely to be kept awake.

PART III.

EXPLANATORY REMARKS ON ALL HIS VARIOUS PRODUCTIONS IN NATURAL HISTORY, ANATOMY AND SURGERY.

June 18, 1772.

ON THE DIGESTION OF THE STOMACH AFTER DEATH. PHIL. TRANS.

JOHN Hunter gives three caſes to prove, that he found a diſſolution of the ſtomach after death: in conſequence of which there is frequently a conſiderable aperture made in that *viſcus* at its great extremity. The three caſes were diſcovered on ſubjects who had experienced ſudden death.

" Being," as he ſays, " employed upon this ſubject, and therefore enabled to account more readily for appearances which had any connection with it; and obſerving that the half diſſolved parts of the ſtomach were ſimilar to the half digeſted food, it ſtruck me, that it was the proceſs of digeſtion going on after death; and that the ſtomach, being dead, was no longer capable

pable of reſiſting the powers of that *menſtruum*, which itſelf had formed for the digeſtion of food."

This paper has not appeared to attract much public attention. But conſidering its importance, it is rather ſingular that it ſhould have remained unnoticed, until an enquiry into the truth of it was undertaken through a train of experiments by the Abbé Spallanzani, whoſe fifth diſſertation is expreſsly for the purpoſe of demonſtrating digeſtion in animals with membranous ſtomachs: the cat—the dog—man—whether digeſtion takes place after death.——

The Abbé ſeems to be a lively accompliſhed phyſiologiſt: he built his experiments upon the practice of Reaumur, and improved them by that natural addition in the application of the labours of two men of ingenuous candour perſevering on a ſubject beyond what one alone could accompliſh. But this was not the ſole merit of the Abbé: ſolidity in judgment appears to have been always conſulted in his experiments: he never undertook them without being previouſly aſſured by the reflection of reaſon, that he was about to practiſe the moſt neceſſary experiment for obtaining the truth of a certain end.

He

He addreſſes John Hunter in an eaſy manner: he ſtyles him one of the beſt anatomiſts of the preſent age; who had frequently found, in the dead bodies which he opened, that the great curvature of the ſtomach was conſiderably eroded, and ſometimes diſſolved. In the latter caſe, the edges of the wound were as ſoft as half digeſted food, and the contents of the ſtomach had got into the cavity of the abdomen;—that ſuch a wound could not have exiſted in life, as it had no connection with the diſeaſe, and more frequently appeared in perſons who died violent deaths. In order to diſcover the cauſe of this phenomenon, he examined the ſtomachs of various animals, both immediately and ſome time after death. In ſeveral he obſerved the ſame appearance. Hence he thought he was enabled to aſſign the cauſe. He ſuppoſes the ſolution to be owing to a continuance of digeſtion after death, and that the gaſtric fluid is capable of diſſolving the ſtomach when it has loſt its vital principle.—From this diſcovery he infers, that digeſtion neither depends on the action of the ſtomach nor on heat, but on the gaſtric juices, which he conſiders as the true *menſtruum* of the food.

When, (ſays the Abbé,) this ſhort but ſenſible paper came to my hands, I was engaged in expe-

experiments on digeſtion; the reſult of which were, that ſome ſubjects were opened ſooner and others later after death; *but among the numbers I inſpected, not one had its great curvature of the ſtomach diſſolved or much eroded.* I ſay much eroded, becauſe I have often ſeen a little eroſion, eſpecially in different fiſhes, in which, when I had cleared the ſtomach of its contents, the internal coat was wanting. The injury was always confined to the inferior part of the ſtomach.—If theſe facts are favourable to Mr. Hunter, a great number are againſt him. And here the Abbé proceeds in a vein of neat ſarcaſtic humour, moſt admirably calculated to let a man ſoftly down.—It muſt have been melting when poured upon the hiſſing hot paſſions of John Hunter,—melting as the wanton flakes of ſnow deſcended into the glowing boſom of a roſy virgin.

Theſe facts, (ſays the Abbé) do not however deſtroy the obſervations; mine are only negative,—his are poſitive; and we know that a thouſand of the former do not deſtroy a ſingle one of the latter, provided it is well aſcertained. I have no reaſon to diſtruſt Mr. Hunter, for his paper has the air of ingenuouſneſs and candour, which uſually accompanies truth."

The

The Abbé modeſtly proceeds to ſay, that the ill ſucceſs of my experiments did not induce me to abandon the idea of digeſtion after death: it only led me to conſider it in another point of view. The reſult of which follows:—

Theſe facts I think deciſively prove, that animals, at leaſt the ſpecies juſt mentioned, continue to digeſt after death. If we conſider the matter rigorouſly, it will be proper to obviate a difficulty that may be ſtarted. However careful we are to kill the animal immediately after it has ſwallowed food, it is certain, that there will be a ſhort interval between the time the food gets into the ſtomach, and the death of the animal, *and that the gaſtric fluids act upon it during this interval: moreover after death they will act for ſome time juſt as in life, ſince the vital heat is not inſtantly extinguiſhed.* The digeſtion therefore obſerved in dead animals may, if not intirely, at leaſt in part, be produced by the gaſtric fluid acting during life, *and a ſhort time after death.*

The Abbé purſued the inveſtigation by farther experiments, which tended to confirm, and which did confirm, the *neceſſity of heat to digeſtion* in many animals. He then concluded this curious enquiry by reſolving to ſee what change

would take place upon fleſh, when the ſtomach was taken out of the body. And he finiſhes the ſubject in the following manner:—

In theſe experiments, I did not perceive any eroſion of the ſtomach, any more than in thoſe made with the view of verifying Mr. Hunter's. I only ſaw, what I had ſeen before, a ſlight excoriation of the inferior part. We muſt therefore infer, that the coats of the ſtomach ſuffer leſs after death than fleſh introduced into it.

Upon reviewing the experiments, and following paragraphs, it cannot, I am apt to think, be doubted, that digeſtion goes on for ſome time after death. I therefore entirely agree ſo far with the celebrated Engliſh anatomiſt, but I cannot with him ſuppoſe, *that this function is independant of heat*; numberleſs facts related in this work fully prove the contrary.—Thus ſays the Abbé.

Theſe experiments and concluſions, drawn from them by the Abbé Spallanzani, produced from John Hunter a paper at ſome length, entitled *Obſervations upon Digeſtion*, throughout the whole of which he evidently diſcovers ſigns of diſconcertion. He begins with moralizing;—he proceeds with *ſcolding like a very drab*;—and he

he finiſhes without producing a *freſh inſtance* of the coat of the ſtomach being found to be diſſolved;—notwithſtanding the idea was gone abroad—the obſervation was open to any one who choſe to ſeek after it—and notwithſtanding the number of years that had elapſed, from the time of John Hunter's firſt paper being publiſhed—the time of Spallanzani's ingenious obſervations upon it—to the time of John Hunter's laſt obſervations.

To what purpoſe was all he could ſay beſides,—if he could not conjure up another caſe to back the three that were diſputed?

Was it becauſe the Abbé had ſaid his firſt paper was ſhort, that he made his ſecond ſo long?

Why has he referred us to his uſual reſource in argument, by recalling attention away from the fact to what he did at Belliſle in the years 1761 and 1762;—and does his cramming the ſtomachs of lizards and worms ſubſtantiate the appearances of ſtomachs in man after death—ſo ſtrongly as examining human ſtomachs?

Does his abuſe of prieſts,—by ſaying that it is preſumption in them to affect to reaſon upon a ſcience in which they have but a ſuperficial

knowledge,—confirm the exiſtence of a phenomenon which he is contending for?

If I can take upon me to aſſert, that ſince the propagation of theſe three caſes given by John Hunter ſo long back as in the year 1772, there has not been *another* diſcovered, where the ſtomach has been digeſted after death,—I think I ſubſtantiate a ground for true criticiſm. And if I can take upon me to aſſert, that there is *no inſtance* to be found of any ſuch caſe, *before* the time of John Hunter's propagation of the idea by three caſes,—I think I am authorized in ſaying, that I do not believe him—that I doubt both cauſe and effect.

I cannot point out any thing more obvious, than that the digeſtion of the ſtomach would appear to the eye of *every obſerver*, if in reality ſuch a fact exiſted. But as it will ever be found difficult to ſee what never happened, I muſt aſſign *that* as a reaſon why the digeſtion of the ſtomach after death has proved, from the obſervations of all men beſides John Hunter, to be a *Nonentity*. The cauſe of its not being ſeen cannot be from ignorance in anatomy or phyſiology: to ſee this phenomenon only requires the uſe of the eyes—only ſuch a uſe of them as might be applied to the confirmation of an object already eſtabliſhed.

If

If the gaſtric fluid, independant of animal heat, after the departure of the vital principle, poſſeſs in its nature a power of diſſolving the ſtomachs of thoſe who die ſuddenly—if the gaſtric fluid can thus diſſolve a ſtomach ſolely by its power, when that fluid was not vitiated, and —when the component parts of that ſtomach were not impaired by long diſeaſe—how much more frequently would the cauſe and effect, be diſcovered on the ſtomachs of thoſe, who lingered till death—and where there was a vitiating power acting upon a part feeble in reſiſtance?

If Spallanzani has proved by experiments that digeſtion after death cannot go on *beyond the time of departure of vital heat*—if obſervations tell us, that excepting to John Hunter, the caſe has not occurred—if reaſon points out that the gaſtric fluid upon a cold lifeleſs ſubject is perfectly inactive—then it follows that the cauſe of thoſe caſes given by John Hunter has not been truly defined: and the ingenuity of phyſiologiſts might be at reſt with reſpect to any cauſe they might ſeek to aſſign for it, until the caſe happen again.—Curioſity need not be awakened at leaſt, until the deſultory evolution of this phenomenon—this eccentric and blazing comet with a fiery tail—be again diſcovered in the anatomical hemiſphere.

OBSER-

July 1, 1773

OBSERVATIONS ON THE TORPEDO.

PHIL. TRANS.

THE torpedo or cramp fiſh, or electrical ray, the raja torpedo of Linnæus, is a flat fiſh much of the figure of a thornback. Its meaſurement in thoſe of a moderate ſize is about four feet in length, and two feet and a half in breadth. A ſpecies of them has been found in the Mediterranean, on the coaſt of France, at Torbay, and at Waterford.

This fiſh was early known to the Greeks, who, from the name they gave it, diſcover that they had a knowledge of its torporific qualities: calling it, ναρκη. Pliny and Plutarch both note it.

Among the moderns,—Reaumur has made experiments upon it, and has communicated them to the Royal Academy of Paris in the year 1714. He deſcribed the muſcles by which the electrical power is conveyed, and was followed by Redi, Steno, and Lorenzini.

Since theſe reſearches, experiments to aſcertain —whether the ſhocks given by the torpedo correſponded with the theory of electricity,—were for ſome

ſome time the favoured purſuits of John Walſh, who publiſhed his paper about the time that John Hunter's anatomical deſcription came forth, and which was read to the Royal Society, at the inſtance of John Walſh. Two plates were annexed to this paper. The experiments of Walſh were continued on by others. Cavendiſh publiſhed alſo the reſult of his experiments in the Philoſophical Tranſactions.

As John Hunter followed Reaumur, Redi, Steno and Lorenzini, in diſſecting the torpedo, *but without noticing their names*—I do not doubt nevertheleſs by his anatomical knowledge he might have left the ſubject improved.

Feb. 27, 1774.

OF CERTAIN RECEPTACLES OF AIR CELLS IN BIRDS.
PHIL. TRANS.

I Have conſtantly endeavoured to make a diſtinction between the merit of him who makes a diſcovery and of him who proſecutes that for farther diſcoveries—which has been already eſtabliſhed. The firſt muſt be conſidered as an active reſult of original ingenuity—the ſecond might be merely the mechanical progreſs of anatomical obſervation.

John Hunter preſented this paper to the Royal Society as containing original information—as the pure reſult of what he himſelf had diſcovered.

The ſubject is truly ingenious and highly curious: and conſidering it both in a natural and anatomical view, it imparts uſeful information.

The cells in the bodies of birds which receive air from the lungs are to be found (in ſome only,) both in the bones and in the ſoft parts; and have no communication with the cavity of the common cellular membrane.

The

The bones which receive the air are lighter, leſs vaſcular, containing little oil without marrow, and weaker than other bones. The Turkey cock, the pouting pigeon, the pelican, the owl, and gooſe, with many more, poſſeſs thoſe air cells,—ſome in the ſoft parts, and—others in the bones, of which I ſhall not go into an explanation. The ſwelling of the Turkey cock,—the pouting of the pigeon,—the hiſſing of the gooſe and of the owl are all explained by a knowledge of this diſcovery.

When John Hunter publiſhed this paper, he, as far as his reaſoning or his prudence ſuggeſted, aſſigned to the lungs of thoſe birds the power and channel of conveying the air into the cells: and what was not done by the channel of the lungs, he ſaid he did not know by what means it was done.

In this paper, he makes it out to be underſtood,—that it ſhould, if it were not really entitled to originality, have all the appearance of it. And in order that this prudent ſuggeſtion might be the better received, he with much plauſibility and ſeeming ingenuouſneſs aims to ſhew that he could demonſtrate ſome parts, but was at a loſs in accounting for others: and thus has he

ſaid, " I do not know by what means the air is diffuſed univerſally to parts."

This *candid* confeſſion of not being perfect in the knowledge of a ſubject newly diſcovered, *ſeemingly* carries with it an air of truth, which would throw the ſhallow miſgiving ſceptic off his guard : and his not quoting a ſingle authority would naturally ſave the trouble of any reference. But John Hunter I know had no delight in comparative anatomy, when it was to be connected with comparative ingenuity ; and the darker he made the room of information, the more he was at liberty to do within it whatever he choſe without being detected.

Next to the importance of this ſubject, as really uſeful in accounting for what would be phenomenon without it, is the queſtion that ariſes upon the originality of the paper publiſhed by John Hunter without a reference to another name. And knowing as I do—that his natural propenſity was for making every thing his own,—that every diſcovery was firſt to be brought within his own vortex,—and afterwards to be diffuſed to the public for their good, without his caring for the real author,—I might perhaps be ſuppoſed to decide againſt judgment and right,—if I were not to aſſign my facts.

When

When the Profeſſor Camper had read this paper in the following year 1775, he inſtantly refers to a paper publiſhed by himſelf, upon the ſame ſubject, and printed at Amſterdam. It was joined to a volume of other phyſiological ſubjects; one of which was, "a ſhort Expoſition of the Anatomy of a young Elephant," &c. Theſe are his words, extracted from a catalogue of all his works, under the article of the year 1775.

In eodem diario extat epiſtola, in qua, me diu ante clariſſ. Joh. Hunterum aëris ingreſſum intra cava oſſa volucrum detexiſſe, evidentiſſime demonſtro; etiam rationem, qua aër per tubas Euſtachianas in mandibulas avium et intra duplicaturam tabularum cranii ingreditur: quam ſe ignorare adhucdum in vol. lxiv. *Phil. Tranſ.* ann. 1774. *edit. faſſus eſt.* pag. 211.

Roterodami 1774. vol. i. p. 235.—*Diſſertatio de Oſſium majorum in avibus ſtructura. Ubi aëris ingreſſum in cava oſſa per reſpirationem demonſtro, et inventionis hujus diem* 10 *Feb.* 1771 *indico.*

In a ſecond paper, written to explain more fully his firſt, John Hunter has thus anſwered the charge of Camper: and it will appear that according to circumſtances he can ſuit the tone

of his voice—the profeſſor having come over to England on purpoſe to clear up the plagiariſm.

" When I wrote this account to ſend it to the Royal Society, I did not then know by what means this was done: for in that I ſaid ' but by what means I do not know ;' that is, I did not know whether it was conveyed by the trachea, where it paſſes along the neck, or the Euſtachian tube. Profeſſor Camper, when he did me the honour to call upon me, was ſo obliging as to take ſome pains to ſhew me, in the lower jaw of the hawk, the hole where the air entered ; which makes me ſuſpect he did not underſtand what I had written. For after having given the marks by which ſuch openings were particularly diſtinguiſhed, it will be hardly ſuppoſed I could ſay that I did not know the hole where the air entered."

March 17, 1774.

ON THE GILLAROO TROUT.

PHIL. TRANS.

THE ſtomachs of trouts are uncommonly thick and muſcular: they feed on the ſhell fiſh of lakes and rivers, as well as on ſmall fiſh; and take into their ſtomachs gravel or ſmall ſtones, to aſſiſt in comminuting the teſtaceous parts of their food.

The trouts of certain lakes in Ireland, ſuch as thoſe of the province of *Galway*, are remarkable for the great thickneſs of their ſtomachs; which, from ſlight reſemblance to the organs of digeſtion in birds, are called gizzards; and the ſpecies that have them are called *gillaroo* or *gizzard* trouts.

Daines Barrington ſuggeſts, that *gillaroo* may be either a corruption of *Killaloe*, the name of a town near to where thoſe trouts are caught; or formed from a Welſh word ſignifying ſtomach and an Iriſh word ſignifying ſtrong, ſo that *gillaroo* might be the ſame as *ſtrong* ſtomach.

Watſon had written on the ſubject before John Hunter, and proved that the digeſtive power

power in this fiſh was carried on in reality by a *ſtomach*; and not, as erroneouſly conceived, by a *gizzard*. John Hunter has only ſaid the ſame at ſecond hand. But in ſaying this, *I beg to be underſtood as not aſſerting that John Hunter has borrowed any of his ideas from Watſon*; or if he did, he has not acknowledged them,—not having mentioned the name of any one throughout his paper.

May 11, 1775.

ON THE GYMNOTUS.

PHIL. TRANS.

THIS fiſh is known more commonly by the name of the electrical eel. It is a freſh water fiſh, found in the river of *Surinam*. The common ſize is from three to four feet in length, and from ten to fourteen inches in circumference about the thickeſt part of the body. It reſembles the conger eel.

This fiſh has been lately much the object of examination. Bancroft, in his eſſay on the natural hiſtory of *Guiana*, has deſcribed it; and Garden had given an anatomical deſcription of the component parts of it in a paper in the Philoſophical Tranſactions before John Hunter; but whoſe name or whoſe progreſs in the enquiry is not regarded by him. Garden's paper is dated from Charles Town, America, Aug. 14, 1774, and contains the beſt deſcription of this wonderful fiſh.

The aſtoniſhing property of this fiſh is its power, exerted at its own pleaſure, of giving an electrical ſhock to any perſon, or number of perſons joining hands, provided the firſt touched the

the fiſh, and the laſt put his hand into the water in which it ſwam.

The ſhock is conveyed either by the immediate touch of the fiſh with the hand, or through the ſame metallic or other conductors which convey the electric fluid; and it is intercepted by the common non-conductors of that fluid. Theſe electric qualities depend upon uncommon large muſcles.

Walſh was proſecuting his enquiries into the nature of this power in the gymnotus at the ſame time that he was in the torpedo, and procured from John Hunter an anatomical deſcription of this alſo.

John Hunter has purely obeyed the invitation of his friend, and has given a perfect anatomical deſcription of the gymnotus with an illuſtration by three admirable plates; with no further comments than what tended to explain the ſubject.

EXPE-

June 24, 1775.

EXPERIMENTS ON ANIMALS AND VEGETABLES WITH RESPECT TO THEIR POWER OF PRODUCING HEAT.

PHIL. TRANS.

JOHN Hunter is moſt commonly induced to make choice of his ſubject, from ſome one having, a little previouſly to the time which he gives it his conſideration, treated upon the ſame. And whenever that is the caſe, he always endeavours to induce the public to believe—that he was in poſſeſſion of the knowledge he has to impart independant of that which others have ſuggeſted. It is from that cauſe, he always refers his readers to notes made at ſome diſtant date, and for the ſame reaſon, he is ever cautious in mentioning names or quoting authors.

Theſe are his motives for beginning this paper after the following manner:—

" Some late ingenious experiments and obſervations, publiſhed in the Philoſophical Tranſactions, upon a power which animals ſeem to poſſeſs of generating cold, induced me to look over my notes, containing ſome which I had made in the year 1766, indicating an oppoſite power in animals, whereby they are capable of reſiſting any external cold while alive, by gene-

rating within themſelves a degree of heat ſufficient to counteract it."

The whole of this paper conſiſts in experiments, and the notes of experiments muſt be a detail of them. The names of Fordyce and Erwin are appealed to as witneſſes to ſome of the experiments : the former was the author of ſome late ingenious experiments and obſervations alluded to by John Hunter; but he, forgetting what he has before aſſerted with reſpect to the dates of his own notes, has confeſſed inadvertently, in a part of his paper, that ſome of his experiments were made many years after, as it can be proved from their dates. I am induced to believe, from the internal evidence of my own underſtanding, that he had never thought of the ſubject until it were thus brought forward with great philoſophical eclat by Cullen, Fordyce, and others.

The experiments made by Fordyce in a heated chamber, on the effects of heat upon the human conſtitution, of which a report was given in the Philoſophical Tranſactions prior to this paper by John Hunter—and the experiments on the power that certain animals poſſeſs of producing cold, made by the modeſt and ingenious Crawford, a report of which was given in the Philoſophical

ſophical Tranſactions ſubſequent, contain perhaps the fulleſt and moſt ſatisfactory arguments on the accommodating temperament of living animals to climates, which have ever been adduced.

But both theſe valuable papers in their purpoſes, go much further in explanation of many phenomena in the animal œconomy, than the limits of my preſent intention will permit me to demonſtrate.

The mechanical philoſophers, particularly Bacon, Boyle, and Newton, conſidered heat as producible in any body: and they were certainly right as to the cauſe of heat. Men with minds leſs comprehenſive have contradicted them from not tracing effect up to cauſe. When de Luc evinced, by a variety of experiments, that the expanſions of mercury between the freezing and boiling points of water correſpond preciſely to the quantity of abſolute heat applied, and that its contractions are proportionable to the diminution of this element within thoſe limits, he cannot be ſaid to have ſhewn a cauſe of heat.

In the application of the cauſe of heat perceptible in animal bodies, mechaniſm, upon the principle of vitality, appears to be the cauſe, and

not denſity, as ſuppoſed by Boerhaave. Subſequent philoſophy has ſuggeſted—that heat is a diſtinct ſubſtance, or an element *ſui generis:* and Crawford has obſerved, that by viewing the phenomena in that light, they will be found to admit of a ſimple and obvious interpretation, and to be perfectly agreeable to the analogy of nature. This may be very fair, as the perception ſtrikes our ſenſes; but ſtill there is beyond that argument an occult cauſe, which has not in my opinion been more ſatisfactorily aſſigned or explained, than by the mechanical philoſophers whom I firſt alluded to.

Whether the ſubject be diſcuſſed under the head of cold or heat, as affecting animals, it is immaterial, both the terms being relative: and therefore John Hunter cannot be ſaid to be more original, becauſe he has varied his title from thoſe who purſued the ſame queſtion, any more than thus,—that—when he treats of heat he means freezing.—He freezes animals to determine, how far the principle of vital heat bears up againſt the influence of cold. His experiments, if they be true, carry with them no manner of information:—if they be true, no effect for the benefit of man can poſſibly be derived from them:—as they were made on reptiles of the loweſt order, and whoſe anatomical conſtruction

ſtruction is *ſui generis*. They amount in number to 44, and were chiefly conducted on dormice and on worms. They will never be read but from curioſity, and will be directly rejected from their trifling inſignificance.

PRO-

March 21, 1776.

PROPOSALS FOR THE RECOVERY OF PEOPLE APPARENTLY DROWNED.

PHIL. TRANS.

JOHN Hunter obſerving that the endeavour to recover perſons apparently drowned was a new practice, and had furniſhed as yet but few important clear facts, takes upon himſelf to iſſue out his inſtructions, ſomewhat after a manner ſimilar to a proclamation.

" Having been requeſted," (he ſays,) " by a principal number of the ſociety eſtabliſhed for the recovery of perſons apparently drowned, to commit my thoughts on that ſubject to paper, I readily complied, hoping, that although I have had *no opportunities* of making actual experiments upon drowned perſons, it might be in my power to throw ſome light on a ſubject ſo cloſely connected with the inquiries which for many years have been my buſineſs and favourite amuſement."

After having peruſed this ſelf approving paper with the utmoſt attention, and remarked its leading inſtructions, I am able to ſay, that in comparing it with what has been advanced by Cogan, Letſom, Kite, and Colman, it falls very ſhort

ſhort of their practical reaſoning ;—and further, that in ſome of his remedies there is cauſe for ſeriouſly ſuſpecting practical miſchief. His double *bellows* does not accord at all with my opinion :—it is not ſuited to the caſe. For if th recovery of drowned perſons conſiſted in procuring the action of the lungs upon the principle of mechaniſm, previous to the action of the heart, and independant of muſcular and every other vital power, yet obſervations made by the matureſt opinions are againſt the propriety and poſſibility of any ſuch promiſing proſpect :—the natural effect from drowning forbids all hopes in it.

This *bellows* of his was ſaid to be made according to his notes in the year 1755, for the purpoſe of trying ſome experiments upon a living dog. It was conſtructed in ſuch a manner, as by one action to throw freſh air into the lungs, and by another to ſuck out again the air which had been thrown in by the former action, without mixing the two airs together.* The muzzle of this *bellows* was fixed into the trachea of a dog, and by working it he was kept alive. I ſhall not contend the facts of the invention of the *bellows* and of the experiments on the *dog* :—of the former being invented and tried upon the latter in the year 1755, and by the record of

* He took the idea moſt probably from the conſtruction of the bagpipes.

of notes then made,—of theſe being ten years afterwards brought forward to anſwer the purpoſes here required.—This is not worthy my time. But the probable inference from this application of the *bellows* ought to be remarked.

Is the caſe of a live dog, as ſtated by him, applicable to the caſe of a drowned man—where the vital powers, the actions of the lungs, the heart, and the muſcles aſſiſting reſpiration, have ceaſed, are ſuſpended, or poſſibly never can be reſtored? and what from obſervation has been the ſtate which the lungs have been found in of a drowned man? An old opinion prevailed that the water by its force and weight ruſhed into the lungs, filled them, excluded the air, and produced death almoſt in the ſame manner, as a collection of water ſometimes does from a diſeaſe of the lungs. But this opinion was confuted by Becker, and in which he was joined by the illuſtrious de Haller.

However de Haller has ſaid, that upon opening a woman drowned in the river *Leine*, and who had remained ſeveral hours under water, he had an opportunity of obſerving what he had previouſly diſcovered by experiments —viz. that all the parts of the lungs and breaſt remaining entire, upon preſſing the lungs the water

water which had made its way into them, evidently regurgitated by the wind pipe; ſo likewiſe, upon preſſing the ſtomach, the water which the woman had ſwallowed returned by the gullet. The lungs were entirely black, and the heart void of blood.

But we ought not to conclude, (de Haller ſays) from this inſtance, that Becker's hypotheſis is not founded on truth: for a very probable cauſe may be aſſigned for it, and both obſervations, of the water being found or not in the lungs, may be reconciled thus:—

If the body, for example, ſhould be opened immediately or very ſoon after the perſon was drowned, there might be perhaps no water found in the bowels, the conſtriction of the *glottis* excluding it.—But if it be not opened till ſeveral hours or perhaps days after death, the tone of the muſcles in that time being relaxed, the *larynx* and *glottis* will both be opened, and the water, eſpecially if the perſon was drowned where it is deep, will, by its proper weight, gradually make its way through the relaxed paſſages into the lungs and ſtomach.

De Haller made experiments for aſcertaining what hopes there may be of recovering perſons

drowned, ſome of whom *have been ſaid* to be brought back to life after lying a conſiderable time in water. And theſe in my opinion are more ſatisfactory to demonſtrate what happens, what is true, and what is proper and poſſible to be done, than any hypothetical conjectures that might be formed by the ſucceſs of a *bellows*.

In the year 1753, two drowned dogs died within twenty five minutes, ſo as not to be recovered by all the arts that could be uſed. There was water found both in the ſtomach and lungs, and by compreſſion it run out by the wind pipe mixed with a great deal of foam. The lungs were red but ſwam in water.

A cat was plunged ſuddenly into water, and died irrecoverably in the ſpace of two minutes. In the ſtomach there was no water, but it had got into the lungs, and run out foaming, mixed with the air of the *trachea*.

In another dog which was irrecoverably drowned, there was a great quantity of water both in the ſtomach and lungs; in the *cava* and pulmonary veins there was a deal of black thick blood.

In

In a fourth dog, which was taken out after remaining *ſeven minutes* under water, and was quite dead, there was a quantity of water found in the ſtomach, and likewiſe in the *trachea* and lungs, mixed with foam: the veſſels belonging to the right auricle and ventricle were full of blood, and thoſe belonging to the left, empty.

From other experiments it appeared, that the *glottis*, in animals that have been drowned, remains open; that the water found in them has been ſwallowed voluntarily, but it does not make its way into the lungs of the dead body by gravity; for in thoſe animals that are plunged into water after they are dead, there is no water found neither in the ſtomach nor lungs.

In all the experiments which the ingenious Evers publiſhed in his *theſis* at Gottingen in 1753, the event was almoſt the ſame as abovementioned.

The cauſe of death, in animals that are drowned, ſeems to be chiefly by the water drawn into the lungs, and by the laſt ſtrugglings of the animal conquaſſated into foam with the air contained in the *trachea* and lungs, which foam cannot be expanded by any dilatation of the *thorax*.

Theſe experiments leave little hopes of recovering perſons who have been drowned, ſeeing that the obſtructing foam cannot be expelled from the *aſpera arteria* and lungs. How far the *bellows* of John Hunter is calculated to remove this obſtructing foam—to diſengage it from the lungs and *aſpera arteria*—to reſtore the ſuſpended circulation of air, blood, and vital heat—muſt be very obviouſly known to thoſe who conſider that a double *bellows* made for the purpoſe of exhauſting or filling a ſubſtance with air, cannot exhauſt from that ſubſtance a *viſcid foam*, nor reſtore vital heat ſuſpended at leaſt if not annihilated.

It might be conſidered as out of ſeaſon, when reaſon even interferes in obſtructing the enthuſiaſm of humanity; and therefore the deluſive hope, founded upon the moſt ſanguine expectation of the recovery of drowned perſons ſhall not be damped by any opinion from me,—where the proſpect is ever ſo diſtant.

Having finiſhed the ſerious part of this ſubject without relieving my mind of a burthenſome reflection which muſt ever attend it—I ſhall only remark farther upon a note of John Hunter's:—

He

He has ſaid—" I ſhall conſider the ſituation of a perſon drowned to be ſimilar to that of a perſon in a trance. In both, the action of life is ſuſpended, without the power being deſtroyed: but I am inclined to believe that a greater proportion of perſons recover from trances than from drowning, becauſe a trance is the natural effect of a diſpoſition in the perſon to have the actions of life ſuſpended for a time; but drowning being produced by violence, the ſuſpenſion will more frequently laſt for ever, unleſs the power of life is rouſed to action by ſome applications of art."

That which he has produced as ſimilar, has been deſtroyed by him for want of ſimilarity.

What is, a Trance?—Compariſons for the elucidation of a truth are generally drawn from familiar ſubjects; at any rate the ſubject for illuſtration by compariſon, ſhould have been defined, by ſomebody, in order to be known. Medically ſpeaking, I have never read of, a trance—hiſtorically, I have heard of it: but it was when I was a child, from the goſſip of old women, as ſomething told at a late hour—as ſomething too af frighting for the houſe maid in a family ever to go to bed without the footman. What author has defined it? Where, I aſk his admirers, am I to look for the information which he was in poſſeſſion of?—

of?—But ſuch were the ſtrong and abſtract powers of the illuſtrious John Hunter!

A Trance has been ever defined as an, Ecſtacy. In this ſenſe, poetically, we can read it in Spencer, Milton, and Thomſon;—but I have never read a medical caſe which authenticates, a Trance, and know not where to find one:—neither Motherby, nor Wallis after him, have given the word a place, even in their *medical* dictionaries.

Locke has aſked—whether, what we call Ecſtacy—be not dreaming with our eyes open? I anſwer—that it is; and refer for an example of my aſſertion—to John Hunter!

1778.

THE NATURAL HISTORY OF THE HUMAN TEETH, AND A PRACTICAL TREATISE ON THE DISEASES OF THE TEETH. IN TWO PARTS. PRICE 1l. 1s.

THIS is a work of public importance, and comes fairly before the criticiſm of a ſurgeon, the ſecond part of it being altogether a performance purely ſurgical.

It is the firſt ripe fruit which has dropped from the tree of his anatomical and ſurgical cultivation. It proclaims an intention of being preſumed to be all his own—of being independant of any borrowed aid—of being the only publication upon the ſubject:—there is not throughout the whole of it a ſingle author alluded to,—there is not a thought ſuggeſted of any one having treated upon it before: it is to be deemed a ſolitary tree of ſurgical knowledge, planted, raiſed, and perfected by John Hunter alone.

In the following treatiſe he alſo profeſſes, that the obſervations were made before the year 1755. He gives a conſequence to the ſubject. He ſays, "that the importance of the teeth is ſuch, that they deſerve our utmoſt attention, as well with reſpect to the preſervation of them, when in a healthy

healthy ſtate, as to the methods of curing them when diſeaſed: that the diſeaſes which may ariſe in conſequence of thoſe of the teeth are various; ſuch as abceſſes, carious bones, &c. many of which, although proceeding originally from the teeth, are more the object of the ſurgeon than the dentiſt; who will find himſelf at a loſs in ſuch caſes, as if the abſceſs or carious bones were in the leg or any other diſtant part."

He proceeds to draw a ſtrong and intereſting or intereſted line of duty between the offices of ſurgeon and dentiſt.

"All the diſeaſes of the teeth, which are common to them with the other parts of the body, ſhould be put under the management of the phyſician or ſurgeon; but thoſe which are peculiar to the teeth and their connections, belong properly to the dentiſt."

He proceeds to ſtate the ſeparate duties of thoſe diſtinct profeſſional offices.—

"It is not my preſent purpoſe to enumerate every diſeaſe capable of producing ſuch ſymptoms as may lead us to ſuſpect the teeth; for the jaws may be affected by almoſt every kind of diſorder. I ſhall therefore confine myſelf to the

the diſeaſes of the teeth, gums, and alveolar proceſſes; which parts having a peculiar connection, their diſeaſes fall properly within the province of the dentiſt. I ſhall alſo purpoſely avoid entering into common ſurgery; not to lead the dentiſt beyond his depth, and to matters of which it is to be ſuppoſed he has not acquired a competent knowledge."

Notwithſtanding what he has aſſerted in the above paragraph, he has been found to treat every diſeaſed caſe according to his beſt ability as a ſurgeon.

He proceeds to explain ſome further intentions.—

"In order that the reader may perfectly underſtand what follows, it will be neceſſary for him previouſly to conſider and comprehend the anatomy and uſes of every part of a tooth, as explained in my natural hiſtory of the human teeth, to which I ſhall be obliged frequently to refer. Without ſuch previous ſtudy, the dentiſt will often be at a loſs to account for many of the diſeaſes and ſymptoms mentioned here, and will retain many vulgar errors imbibed by converſing with ignorant people, or by reading books in which the anatomy and phyſiology of the teeth

are treated without a ſufficient knowledge of the ſubject."

This laſt paragraph is in the higheſt ſtyle of a great and learned author, and it flatters my conſequence inaſmuch as he has flattered his own: for if his work be of that importance, it reflects an importance upon him who criticiſes it: it is —*dignus vindice nodus*—and it ſhall have the faireſt juſtice.

I ſhall begin by avowing an axiom which I doubt if it can be fairly controverted:—that no man can ſit down to write the life of another without being capable of tracing his motives. For if the general motives be not marked which lead on to the great variety of undertakings the opportunity of life is conſtantly offering,—if the motives be not truly aſcertained,—they might be ſeen indeed in many views, but it is very rarely, without that clue which leads directly to the knowledge of the heart, be in the poſſeſſion of the biographer, that he ever can be capable of developing their origin. A laudable motive not being well underſtood,—by conſidering it abſtractedly,—by looking at it aſkance, might be ſhamefully perverted. I therefore ſhall firſt of all, trace this motive in John Hunter for writing this book.

John

John Hunter, at the time he publiſhed this book, had but very little practice; the whole circle being then filled up by names to which I have before alluded. Independant of his half pay as an army ſurgeon, his whole ſupport depended upon profeſſional emolument. It is not to be wondered at therefore, that when the common field of practice was already ſtocked, and its paſturage ſhort, a man of his mental reſource and active application, ſhould be directed in ſearch after freſh ground, where he could by himſelf range at large.

In this motive, he followed in ſome meaſure the practice of the French ſurgeons, but it was directly againſt the habits of the Engliſh. Hawkins, Bromfield, Sharpe, and Pott, were proud and unaccommodating profeſſional men. They were above ſubmitting to conſultations with dentiſts. Their patients who wanted advice for relative complaints of the teeth, ſent for or went to them, and from them took the inſtructions which the dentiſts were to obey. Such was the rude and gothic ſtate of practice, when this motive offered to John Hunter.

He laudably condeſcended to accommodate himſelf to the neceſſity of the caſe:—and to fill up this chaſm in practice, thus proudly kept

open till now, he placidly attended on fixed days and hours at the houſe of a dentiſt,—to aid him by conſultation for the benefit of his patients. But few are amongſt the happy favourites of fortune! John Hunter was not found to beſtow his ſmiles upon every dentiſt;—his ſincerity in friendſhip confined him alone to the family of the Spences. This inſtance of friendly generoſity and partial attachment, is too well known to be rendered doubtful; as ſooner or later there will ſcarcely be found a family in England, that cannot, from meeting him there,—but what is more impreſſive,—from ſharing in his chirurgical bounty, atteſt its truth.

The following note therefore, taken from the preſent work, p. 90, cannot be conſidered as corroborating a fact already eſtabliſhed upon the memory of the public; but it will tend to prove what might be otherwiſe doubted by ſome, viz. John Hunter's ſincerity in friendſhip for the family of the Spences,—as he has here publicly avowed it. After dealing out his cautions to the dentiſts at large "not to extract the teeth too quick," ſomewhat after a ſimilar manner which *Hamlet* delivers his caution to the *Players*, he thus is found to diſtinguiſh the family of the Spences. "I muſt do Mr. Spence the juſtice to ſay, that this method appears to be peculiar to him, and that

that he is the only operator I ever knew, who would ſubmit to be inſtructed, or even allow an equal in knowledge; and I muſt do the ſame juſtice to both his ſons."

In recollecting the name of Spence the elder, an incident comes over my memory which I cannot refrain from explaining. I have ſcarcely introduced a ſtory in this life, and have therefore a greater right of claiming the attention;—eſpecially as this is not altogether epiſodical, but leading directly to the point I am to eſtabliſh,—and more eſpecially as it ſhews, that talent will always make its way, if he who poſſeſſes it looks ſtrait forward to obtain that reward which diſtinction will always pre-eminently exact.

When I was an apprentice in Hatton-ſtreet, in the year 1762, a painful tooth provoked me to have it drawn; and there was no one ſo high in fame for extracting teeth, as the elder Spence. To him I went into *Grays-Inn-lane*, and although he was ſituated in the vileſt neighbourhood, I found not only the neateſt appointments in his profeſſional line, but alſo an operator which commanded the higheſt reſpect from his figure and his conduct,—from his perſonal demeanour and the appearances of things in general.

His

His ſhop was exquiſitely neat. The barbers blocks were as white as ſoap-ſuds could make them, and the blood baſons were as ſhining, as if they had been directly brought home from the ſcowerers. The teeth exhibited as ſpecimens in the ſhop, were as white and poliſhed as ivory: they were developed of every periſhable attachment; and the only wonder was—how they came to loſe their deſtined homes, and how they were found where I ſaw them.

There was a painted hand in the window with ruffles pendant over the wriſt—the lace of bruſſels imitation—holding a tooth betwixt the fore-finger and thumb:—the figure of the hand—the graceful air of the finger and thumb—the pattern of the tooth—and the point of bruſſels lace, were ſo highly finiſhed by the artiſt, that Denner even would have adjudged himſelf to be outdone if not diſtanced both in the deſign and execution. The civility of Spence was beyond all expreſſion.—

After I had gone through the dreaded proceſs of having my tooth extracted, and after that ſort of explanation which ever takes place between the profeſſional man and patient had been fully diſcuſſed, I was moſt kindly introduced into a back room:—there I was ſurprized at finding that a con-

a connection betwixt the philoſopher and the man of an ingenious profeſſion was only ſeparated by a partition of board. The floor was covered by the freſheſt baize, green as the carpet of nature in the month of May; and upon a ſhining mahogany table, there was placed what I had never feaſted upon with my eyes before,—an electrical machine. At that time its rarity enhanced its eſtimation.—Franklin and Ingenhouzen had not made their improvements notorious by publicity: the progreſs of electrical invention was juſt upon the dawn: from what I have ſince recollected of this machine, it was made upon the principle of the Abbé Nollet's—the conductor being ſuſpended to the ceiling by ſilken chords. He excited my wonder, by the dancing of figures,—ringing of bells,—attraction of hair and feathers—and firing gun-powder placed out in the yard. Upon taking my leave, he ſhook me warmly by the hand, and ſaid, young gentleman, we profeſſional men never take any thing from one another! When I came home I found a Hare juſt ſent to me by my father,—and from the impreſſion of gratitude and the high entertainment I had received, was inſtantly induced to carry the Hare myſelf to my generous benefactor.

A man need not have been endowed with the power of prophecy, to foretell that the region of

Grays-

Grays-Inn-lane would not long be honoured with an inhabitant of Spence's promiſe:—in a few years after, the beſt houſe in *Soho-ſquare* was ornamented by him; and before the date of this work of John Hunter,—a profeſſional coalition betwixt them was adjuſted.

Spence was a diffident man: and by the change which he made of ſituation—by the eminence which reputed merit had conducted him to—he experienced a ſort of revolution in practice, which his modeſty diſpoſed him to ſhrink at. He found an alteration in the nature of his practice:—he was not only to diſpoſſeſs the rotten tenants of their manſions, but was now engaged in adorning the ſound ones—in arranging thoſe which intruded upon each other—in filing, cleaning, poliſhing—and at length in tranſplanting. He did not therefore reject the proffered aid of John Hunter. The coalition was formed without difficulty betwixt two men, who laudably aſpired in their diſtinct provinces;—who were born in the ſame country,—and who were, much upon a par in their education; and the mutual intereſt it promiſed to create, gave vigour to that ſcheme, in which John Hunter more than Spence was to be exalted.

John

John Hunter was to produce this work, which would authorize Spence to introduce him as a choſen ſurgeon beſt acquainted with diſeaſes of the teeth and jaws, becauſe he had written a *learned treatiſe* upon them, and had made them his favoured ſtudy. Spence ſoon found his houſe crowded by all the faſhion of the age: the ſtock of beauty even to this day has experienced an encreaſe by their conſultations: and had it not been for a few unfortunate caſes, where by tranſplanting of teeth the venereal diſeaſe was conveyed, and where the patients thereby fell victims to that poiſon, in all probability neither of them would have been diſgraced by the connection.

Almoſt all anatomiſts have written on the teeth, but there are few who have treated it from their own obſervations: they have all ſervilely copied merely each other, excepting Euſtachius, Fallopius, Columbus, André Dulaurens, Rolpinkius, Duverney, Bertin, Albinus, and ſome profeſſed dentiſts. John Hunter, I repeat it again, has not alluded to a ſingle name throughout his book—but that of Spence. This work by him contains little originality, as the following

remarks will fully demonſtrate, and that it was not written from practical obſervation. He very properly begins the ſubject with a deſcription of the maxillary or jaw bones, and firſt of the upper. But he muſt certainly have ſuppoſed his reader to be already acquainted with anatomy, from the very curtailed deſcription he has given of thoſe bones, andthe omiſſions of importance he has committed. It is highly requiſite for a dentiſt to know in what manner the teeth are ſupplied with blood and nerves, and how theſe are conducted to the teeth, as they certainly are as much connected with the teeth, as their *perioſteum* or gum; for by their means ſome very conſiderable changes are brought about.

He has taken no notice, in his deſcription of the upper jaw, of thoſe cavities firſt mentioned by Fallopius—of thoſe maxillary ſinuſes or *antra* by Highmor;—although in the ſecond part of his work he treats on one of the diſeaſes to which the membrane lining thoſe cavities is liable, namely ſuppuration or abceſs. Of theſe the uninformed dentiſt muſt therefore be at a loſs to know where their ſituation is, and of courſe their ſuppuration or abceſs. He has however been particularly full in his deſcription of the alveolar proceſſes

proceſſes—the articulation of the lower jaw—and the motion of its joint and of its muſcles.

He has ſaid, at the bottom of p. 36,—"We cannot by injection prove that the bony part of a tooth is vaſcular: but from ſome circumſtances it would appear that it is ſo, for the fangs are liable to ſwellings ſeemingly of the *ſpina ventoſa* kind, like other bones; and they ſometimes anchyloſe with the ſocket by bony uninflexible continuity, as all other contiguous bones are apt to do." He continues—"But there may be a deception here; for the ſwelling may be an *original formation*, and the *anchyloſis* may be from the pulp which the tooth is formed upon being united with the ſocket."

There was no reaſon for imagining that theſe particularities aroſe from an *original formation*. John Hunter ſeems to have forgotten, that every ſoft part of the body, by a proceſs of nature may be converted into bone. The inſtances of oſſification in the membrane inveſting the cavity of the thorax and in the arteries are numberleſs. The *perioſteum* with which the whole of the fang of a tooth is inveſted, and which is the common covering both of the tooth and its alveolar proceſs, is ſuſceptible of inflammation;—this, like that of any other part, may go on into ſup-

 puration,

puration, and a gum boil, when a part of the ſocket has been deſtroyed, will appear externally: or this membrane may be preternaturally thickened, and a fleſhy kind of ſubſtance be produced, which may in the end degenerate into bone, and which is confirmed by the following facts.

The *perioſteum* covering the ſtumps of teeth, and thoſe of decayed teeth that have been painful from their veſſels being expoſed by *caries*, is invariably found conſiderably thickened. In ſtumps eſpecially which have remained for ſome time in their ſockets, after the crown or body of the teeth to which they belonged has crumbled away, a bony thickening or ſwelling of their extremities is moſt commonly obſerved. The *exoſtoſis* of teeth ariſes therefore from an oſſification of the *perioſteum*, which is ſometimes ſo extenſive, that all the fangs of a tooth will be united together by this proceſs, and which is readily diſtinguiſhed from an original union of the fangs, by its rough and unequal appearance, and then a new *perioſteum* is formed.

A remarkable caſe of *exoſtoſis* of teeth, was ſeen in a man who had not a ſingle tooth in his head, whoſe fang or fangs were not thus affected. Dying of a fever, his teeth were drawn by a ſur-

a ſurgeon,—not with any view to diſeaſe, for he was unacquainted with the man's former ſufferings in his jaws,—but obſerving this ſwelling of the fangs of all the teeth extracted, he was induced to enquire,—whether he had not had a painful affection of his face; and his caſe being remarkably obſtinate, was well recollected. Even the fangs of the *inciſors* of the under jaw were thus affected, although it is very rare to ſee them ſo; and which aroſe perhaps from the veſſels ſupplying them being ſo very minute, and from the *foramina* through which they muſt paſs being alſo ſo minute,—that whatever irritation there may be, it ſeems to have but very little effect in impelling a more than uſual determination of blood to the teeth.

That an *anchyloſis* may take place between the tooth and its ſocket, either at its formation or afterwards, I cannot pretend to deny, but it is a very rare occurrence indeed. It has been enumerated among one of the circumſtances rendering the extraction of teeth difficult; but as far as I can learn from perſons who have paid very particular attention to the teeth, they have never ſeen any thing of the kind. They have known a tooth ſo intimately blended with the ſocket, by means of the *perioſteum*, as to be difficult to determine, whether it was not by a bony union; but

but by macerating ſome time in water, it has been ſeparated, when the membrane has been by putrefaction found to be deſtroyed.

In p. 39 and p. 40, John Hunter has made an aſſertion which ſeems to be not founded in truth—"That affections of the whole body have leſs influence on the teeth, than on any other part of the body: thus in children affected with the rickets, the teeth grow equally well as in health, although other bones are much affected; and hence their teeth being of a larger ſize in proportion to the other parts their mouths are protuberant."

It has been fully and generally known, that rickety children are conſiderably longer in geting their firſt teeth than others, and that they have their ſecond ſmaller than common. As this is the fact, and as the teeth appear above the gum in proportion to their progreſs to perfection, ſo this obſervation by John Hunter muſt be a conjecture in his theory. He was led to it, perhaps, from remarking that weak and puny children generally get their teeth ſooner, and have their ſecond teeth larger and ſtronger in proportion to their other bones,—a fact which cannot be ſatisfactorily explained: it has been plauſibly conjectured, that from the arteries of

of the body being more relaxed in ſuch children, and conſequently thoſe likewiſe ſupplying the teeth, a greater quantity of the matter which forms the teeth is permitted to permeate them. But if this were the caſe, other bones would likewiſe develop themſelves in the ſame proportion, which does not appear to be the truth.

In p. 41, where the cavity of teeth is treated on and its contents, he has not noticed the membrane which lines the cavity, upon which blood-veſſels have been ſeen ramifying in teeth, that have been minutely injected; nor has he mentioned it under the article of *perioſteum*. In p. 47, he ſays—"I chuſe to divide the teeth into the four following claſſes. *Inciſores*, commonly called the fore teeth; *cuſpidati*, vulgarly called canine; *bicuſpides*, or the two firſt grinders; and *molares*, or the three laſt teeth."

Ancient anatomiſts named all the teeth, the canine only excepted, from their uſe: hence the Greeks termed the *inciſores*, τομεοι, and the Romans *canini*, from the ſuppoſed reſemblance they have to dog's large lateral teeth; but they ſhould have been named as all the others have been, ſo as to give ſome idea of their uſe; then they would have been termed *laceratores*: as when we want to tear any thing, we inſtinctively place it be-

tween

tween thoſe teeth; and the reaſon is obvious, becauſe they have longer fangs than any of the reſt, conſequently are more firmly fixed in the jaw-bones, and are more adapted to be oppoſed to any force.

Molares, from their action on the food, are properly named:—is there then ſufficient reaſon for altering the names of theſe teeth to thoſe of *cuſpidati* and *bicuſpides*, becauſe when the former are firſt formed they have one point, and the latter two? Surely there is as much neceſſity for changing the names of the others from their having a certain number of points, as for changing the names of theſe. Of late years a ſpirit of innovation ſeems to have prevailed, for no better reaſon, than to render the knowledge and ſtudy of ſcience more intricate and difficult of attainment. If terms long eſtabliſhed and univerſally received are to be expoſed to capricious changes, ſcience muſt be perpetually involved in perplexity and confuſion.

It has been confidently aſſerted, that no work contained a particular deſcription of the teeth but this by John Hunter. It is very true that he has named no author, and therefore ignorance might be miſled. Euſtachius has been particularly full upon this ſubject; and whoever will be at the pains

pains of comparing the one with the other, will ſoon be convinced that the difference conſiſts only in the collocation of the ſentences. Some French authors have alſo been very explanatory upon teeth, and have not failed to inculcate,—that to be a good dentiſt, it was neceſſary to know the teeth ſingly and ſeparately when they were out of the mouth.

In p. 60 he mentions the maxillary *ſinus*,—that the firſt and ſecond grinders of the upper jaw are placed immediately under it; and in p. 63 he ſpeaks of the *antrum highmorianum* without noticing, that theſe cavities are known by both thoſe names;—ſo that the uninſtructed reader muſt conſequently conclude the latter to be ſome other cavity in the jaw.

Of the formation of the jaws and teeth in the *fœtus*, a very accurate account is given *in libro ſecundo Annotationum Academicarum Albini*; to this I refer the reader for full information. In p. 79, treating on the milk teeth, John Hunter has ſaid,—"Theſe twenty are the only teeth that are of uſe to the child, from the ſeventh, eighth, or ninth month, to the twelfth or fourteenth year."

If theſe were the only teeth uſeful, why ſhould others appear before that time? The firſt adult

molares appear at the ſeventh year,—five years before the earlieſt period he has mentioned. And as ſoon as the fourteenth,—nay even at the thirteenth,—the ſecond *molares* make their appearance. Theſe facts may eaſily be proved beyond the defence of his *admirers*, by watching the mouths of children at theſe ages.

In p. 84 he has ſaid, "that the firſt adult *molaris* comes to perfection and cuts the gum about the twelfth year." I have already ſaid that it perforates the gum long before this period. It is not completely formed at this time, but is much ſooner than the twelfth year,—the time limited by him. This ſhews how little his attention was to the ſhedding of the teeth, or he could not have avoided knowing that the firſt adult *molares* make their appearance in both jaws with the permanent *inciſores*,—about the ſeventh year, when children uſually begin to ſhed their teeth. The ſecond *molares*, which he ſays cut the gum about the eighteenth year, generally appear through the gum the thirteenth or fourteenth; rarely later than the latter period.

I have known the *dentes ſapientiæ*, particularly in females, appear as ſoon as the ſeventeenth year; but more commonly not till the twenty firſt or twenty third year. He makes them appear

pear from the twentieth to the thirtieth. With regard to their coming to perfection, it is ſome time after they have cut the gum, that their fangs are perfectly formed, though they are not ſo late before that happens, as he has obſerved them to be. It very frequently occurs that a tooth will be completely formed, which does not appear in the cavity of the mouth: this is often the caſe with the *dentes ſapientiæ*, and where there has been ſufficient room for them. When this happens, the ſocket fills up, and in proportion as it does ſo, puſhes the tooth into the mouth: and this explains—how teeth in one or other of the jaws, by their opponents being extracted, become longer,—and how ſtumps are protruded.

In p. 93 he has ſaid,—"How the earthy and animal ſubſtance of the tooth is depoſited is not perhaps to be explained." If we may reaſon from analogy, it is explicable by ſaying, that the arteries have that power.

In p. 96 he has ſaid, when treating on the enamel of teeth,—"It is a calcareous earth, probably diſſolved in the juices of our body and thrown out from thoſe parts which act here as a gland." It will be almoſt needleſs to obſerve upon this, that earth is not ſoluble in any *menſtruum*, and therefore this cannot be the truth of

the caſe. But that calcareous earth may be carried into the blood, in ſuch ſmall particles as to be capable of permeating the moſt minute veſſels, and by them depoſited on particular parts,—it cannot be queſtioned.

In p. 99 he has ſaid,—" I have ſeen two or three jaws where the ſecond temporary grinders were ſhedding in the common way without any tooth underneath, and in one jaw, where, in both the grinders, I met with the ſame circumſtance." He afterwards gives the caſe of a lady, who deſired him to look at a looſe tooth not yet ſhed, which he directed to be drawn out, as another might come in its place, which did not ſo turn out. And in p. 100 he has ſaid,—" Theſe caſes prove evidently that in ſhedding, the firſt teeth are not puſhed out by the ſecond ſet, but that they grow looſe and fall out of their own accord:" and then he ſays directly " That the ſucceeding teeth have ſome influence on the ſhedding of the temporary teeth is proved by thoſe very caſes; ſince in one of the firſt mentioned, the perſon was about twenty years of age, and the other lady was thirty, and it is reaſonable to believe that the ſhedding of the teeth was ſo late in thoſe inſtances, from the want of the influence, whatever it is, of the new tooth."

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The ſhedding teeth do not grow looſe, and fall out of the mouth of their own accord; but are always influenced in this reſpect by the ſhooting up of the permanent teeth, and which the following caſe will tend to evince.—A young gentleman had the lateral *inciſor* of the ſhedding teeth placed between the firſt and lateral one of the ſecond ſet, on the right ſide of the upper jaw at the age of ſixteen; which being drawn to give the adjacent teeth an opportunity of approaching each other, as the *dentes ſapientiæ* made their appearance, there was not the leaſt ſign of any waſting of the fang viſible, which doubtleſs there would have been, if the milk teeth did grow looſe and fall out ſpontaneouſly. But there are many other inſtances of this fact, which it may be needleſs to enumerate. In the inſtances which he has mentioned, and which he ſays prove that the ſucceeding teeth have ſome influence,—or thoſe teeth would not have remained ſo long after the uſual period,—there was no opportunity of knowing whether there were teeth under thoſe that became looſe; it is moſt probable that there were, or the ſhedding ones would not have been looſe.

It every now and then occurs, from ſome unknown cauſe, and it ſeems to have been ſo in the caſes he has mentioned, that a tooth will be arreſted

arreſted in its growth and remain ſtationary for a length of time, when from ſome incidental ſtimulus, it will become completely formed and make its appearance.—The following caſe is an inſtance of this:—a robuſt gentleman of the univerſity of Oxford, at the age of forty three, had not ſhed the *cuſpidatus* and the two *molares* on the right ſide of the upper jaw; the *cuſpidatus* and firſt *molaris* were knocked out accidentally by the oar of a boat, and the ſecond ſo looſened, that he removed it with his finger and thumb. In the ſpace of two years from that time the permanent *cuſpidatus* and *bicuſpides* made their appearance.

In p. 113, treating on the ſenſibility of nerves, John Hunter has ſaid—"Nerves of the teeth would ſeem to be more ſenſible than nerves are in common, as we do not obſerve the ſame violent effects from any other nerve in the body being expoſed either by a wound or ſore, as we do from the expoſure of the nerve of a tooth."

All nerves poſſeſs ſenſibility alike, but communicate to the *Senſorium commune* a greater or leſſer degree of pain when morbidly affected, according to the reſiſting power of the parts on which they are diſtributed. Hence we do not feel that intenſe degree of pain in inflammatory affect-

affections of the *viſcera*, the liver and lungs for example, as we do in thoſe of the moſt ſolid parts; and hence the very ſharp pain in the tooth-ach is cauſed by the ſtimulus of cold determining a preternatural quantity of blood to the cavity of the teeth;—for the bone not giving way, that excruciating pain is felt from its reſiſtance. Pregnant women from the ſame cauſe are tormented with pain in almoſt all their teeth, without having them at all affected by a *caries*, and which uſually ceaſes upon their looſing blood.

In p. 116, treating on the irregularity of teeth, he has ſaid,—"This happens only in the adult ſet of teeth, and is owing to there not being room in the jaw for the ſecond ſet, the jaw bone being formed with the firſt ſet of teeth and never encreaſing afterwards; ſo that if the adult ſet does not paſs further back they muſt overlop each other and give the appearance of a ſecond row."

To ſuppoſe that the jaws do not encreaſe in thoſe caſes after the firſt ſet of teeth are formed, is highly ridiculous, and contrary to truth. If it were ſo, the teeth of the ſecond ſet would invariably be irregular; for their magnitude makes them always in every jaw ſo very different from thoſe of the firſt ſet, that there never could have been

been an inſtance of the ſort. Permit me to aſk, —to where the teeth are to paſs further back, i fthere be no room for them in the jaws?

In p. 121, treating on the decay of teeth, he has ſaid,—"From what cauſe this proceeds is hitherto unknown."

It is not for me here to treat on the cauſes of decay of teeth, which are certainly known,—which are obvious,—and which neither call for my diſcernment nor reflection.

In p. 122 he has ſaid,—"It is beſt to draw a tooth on that ſide where the alveolar proceſs is weakeſt."

To a perſon who knows how to fix his inſtrument, it is immaterial on which ſide he does it. But John Hunter's advice encourages fracturing of the alveolar proceſs, which ſhould be always avoided, becauſe the gum, when it does happen, is generally lacerated and ſometimes torn from the necks of the adjacent teeth, and their alveolar proceſſes; in conſequence of which, an exfoliation of the denuded bone will take place, in about ſix weeks or ſometimes much longer from the operation. Surely an injury of ſuch a deſcription ought if poſſible, to be avoided.

It

ON TRANSPLANTING TEETH.

It will be neceſſary before I make any remarks on what John Hunter has ſaid reſpecting the tranſplanting of teeth, as the world in general has been perſuaded to believe it to be a new opeation, to acquaint it that the learned and faithful obſerver in ſurgery, Ambroſe Paré, who wrote on the latter part of the fifteenth century, has made mention of the tranſplanting of teeth, lib. 16. cap. 26.

Firſt of all he gives a caſe where he returned three teeth into their ſockets which had been knocked out, and which faſtened ſucceſsfully: and then he follows with a report of a ſucceſsful caſe of the tranſplanting of a tooth. But he did not of himſelf perform the operation, nor was he preſent when it was performed. He ſays of it,—*Auditum habeo ab homine fide digno.* But as ſeeming to have his doubts reſpecting it, he cloſes the caſe with adding,—*ſed ut jam dixi de hac re, nihil præter auditum habeo.*

From amongſt ſeveral more authors, Mauquet de la Motte, *Traité de la Chirurgie, t. i. obſ.* 2. has related, in the reflections he was making upon what had been his remarks on caſes of teeth,

which were drawn and replaced in their ſockets, by obſerving firſt,—that it is prejudicial to draw a tooth that is not decayed, and the pain of which depends only on the irritation of the membrane enveloping the root of the tooth. He adviſes, if a tooth under ſuch circumſtances be removed, that it be reſtored ſpeedily to its place, *puiſque reprend aisèment*. He has given a caſe to prove, that a tooth thus extracted and again reſtored, will, if it faſtens, partake of all the ſenſibility of a tooth untouched.—It was of a gentleman who had a tooth drawn, and upon finding it ſound, had it returned to the ſocket directly, and it united perfectly well. He hoped that by the nerve at the bottom of the ſocket being broken, he ſhould have been hereafter free from pain of this tooth, but was miſtaken, as in ſome years after, he was cruelly tormented with pain in it, inſomuch as to have the ſame tooth again drawn. This was with difficulty effected after many tugs, and in extracting it a portion of the lower jaw bone came away with it.

Fouchard, p. 342, has given a caſe of the ſenſibility of a tooth, after it was tranſplanted from the mouth of a ſoldier. It was in the year 1715: the name of his patient was Romalet, captain in the ſecond battalion of Bourbonnois. It remained in, for ſix years, and was drawn for being

caries.

caries. During the diſeaſe it frequently gave pain, particularly when the ſtump was filled up with lead.

Theſe inſtances one would imagine are ſufficient for proving the antiquity of the practice of tranſplanting teeth. And as all of them are dated prior to the *nativity* of John Hunter, he therefore could not poſſibly refer back to his *notes*, as was his practice in almoſt all other caſes, for the purpoſe of being ſuppoſed the original author of tranſplanting of teeth. But if it be true, as he has ſaid, that he never did read, probably it might notwithſtanding have been his own original ſuggeſtion.

In p. 127 and p. 128, he is found to aſſert what is contradicted in the ſecond part of his work. Treating on the tranſplanting of teeth,—"In like manner a freſh tooth, when tranſplanted from one ſocket to another, becomes to all appearance a part of the body to which it is now attached, as much as it was of the one from which it was taken; while a tooth which has been extracted for ſome time, ſo as to loſe the whole of its life, will never become fixed. The ſockets will alſo in this caſe require the diſpoſition to fill up, which they do not in the caſe of the inſertion of a freſh tooth." Whereas in p.

105 he has ſaid,—"The inſertion of dead teeth has been recommended, and I have known them continue for years without any alteration."

This is an operation which has been likewiſe formerly practiſed, and many credible inſtances are recorded of its proceſs. The practice however of both ſeems now deſervedly to be diſcontinued: that of tranſplanting the teeth, on account of the diſeaſe which may be communicated by it: and without a boaſt of any other pretence than duty, I am to tell, that in its *baniſhment out of practice*, I have been profeſſionally inſtrumental. In the thirty laſt pages of my obſervations upon John Hunter's treatiſe on the venereal diſeaſe, I have eſtabliſhed a caſe which no authority can deſtroy, of a lady who fell a victim to the venereal diſeaſe in conſequence of a tooth being tranſplanted from the mouth of an *infected* girl into her own.

That of the inſertion of a dead tooth ſeems to be diſcontinued from want of ſucceſs: and as the method of fitting in natural teeth is arrived to ſuch perfection ſo as to remain firm, when there are fangs for them to be inſerted upon, ten years or more, where can be the neceſſity of tranſplanting teeth;—an operation, independant of infection, the ſucceſs of which is uncertain?

Diſeaſes

Diſeaſes of the teeth have engaged the attention of almoſt every writer on ſurgery both ancient and modern : and as far as my obſervations have gone, they have been treated on in a more conciſe, ſatisfactory, and erudite manner, than in the work now before me. Of this I can ſpeak with unbounded confidence, that the modern ſyſtem of ſurgery would arraign many parts of John Hunter's practical treatment of diſeaſe as found in this treatiſe; and that he has not reached by many degrees the preſent ſtandard as univerſally adopted.

Much remains to be corrected, and much to be yet done, in this part of ſurgery, which has been too much neglected, and which it is reaſonable to ſuppoſe may be in ſome meaſure attributed to the work now before me; as competition is naturally arduous, but becomes more ſo in the opinions of young writers, when they think they are engaging in a conteſt with a *giant in fame*.

OBSERVATION ON P. 111.

John Hunter has ſaid,—that he tranſplanted a tooth from a perſons head and faſtened it with threads on the comb of a cock. The cock was killed

killed ſome months after, when he injected the head. The comb was then taken off and put into a weak acid, and then he ſlitted the comb and tooth into halves. He found the veſſels of the tooth well injected. But in a *note* to this he adds,—" I may juſt remark that this experiment is not generally attended with ſucceſs. I ſucceeded but once out of a great number of trials."

This *note* was extremely neceſſary, and it enhances the value of the preparation. And I may juſt remark alſo, that if this preparation is to be found in his *muſeum*, it is that which I ſhould have been the proudeſt of,—it being ineſtimable: and as one can hardly ſuppoſe that he would have thrown ſuch a *pearl* away, it muſt of courſe be now ſeen in a conſpicuous part of the *muſeum*. But if it be not, I am afraid that the whole will be ſuſpected as a fabrication; eſpecially as he has ſaid, in p. 36,—" We cannot by injection prove that the bony part of a tooth is vaſcular :" therefore inſtead of this experiment being ſeen in p. 36, it is ſent out of the way from the true aſſertion there, to p. 111.

The plates to this work are moſt excellent.

AN

Feb. 25, 1779.

AN ACCOUNT OF THE FREE MARTIN.

PHIL. TRANS.

FREE Martin is a name given in this country to a cow calf, caſt at the ſame time with a bull calf, which has been conceived to be a kind of hermaphrodite,—that is, never known to breed, nor to diſcover the leaſt inclination for the bull; —nor does the bull ever take the leaſt notice of this animal. It has all the external marks of a cow calf, namely the teats and the external female parts, called by farmers, the *Bearing*. When theſe animals are preſerved,—it is not for propagation, but for all the purpoſes of an ox or ſpayed heifer,—to yoke with the oxen and to fatten for the table. They are much larger than either the bull or the cow; and the horns grow larger, being very ſimilar to the horns of an ox. The bellow of the free martin is like that of an ox; and the meat reſembles that of the ox or ſpayed heifer, being generally, as it is ſaid, finer than that of the bull or cow, and is more ſuſceptible of growing fat with good food.

John Hunter has given an anatomical deſcription of three of thoſe animals; and according to his account there does not appear to be any ſyſtem

ſyſtem obſerved by nature in their formation; for if there were, they would all of them have correſponded with each other; whereas on the contrary, they all of them have differed moſt materially from each other, at leaſt according to the relation which he has given of the appearances of parts of generation which offered from his diſſections.

If theſe diſſections have been made by him, and if he has honeſtly reported them, his concluſions from them are very lame. For I am apt rather to preſume, that the ſame uniformity in appearances of every free martin externally, is an indication of their internal conſtruction being uniform alſo. Natural hiſtory has correctly ſtated the external appearance of every free martin to be uniform; and John Hunter has not ventured to contradict it. Whereas he alone, as far as I know, has given an anatomical deſcription of the parts of generation; and in that deſcription is ſeen a jumble of the ſexes confounded together; and that confuſion in each is ſeen to be varying from the other. Now if this be the fact, and as he has ſtated it, a free martin cannot be ſtrictly termed an hermaphrodite, but a *luſus naturæ*. Every one, according to him, is a *luſus naturæ*.

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I believe the time will come, when it will be made apparent—that an imperfection in the organs of generation, either of a male or female animal in general, will no longer be ſaid to be owing to both the ſexes being confounded together. In all thoſe ſingular caſes which have come before my obſervation, and which have been deſcribed as hermaphrodites,—they have proved to be nothing more than an imperfect formation in the parts of generation of one or other of the ſexes: for I never yet have been able to trace a ſolitary inſtance of both being confounded together. And in this inſtance of the free martin, I ſhall only add, that if upon diſſection there be a variation in anatomical appearances, the inference is fair in preſuming, that the external figure of the animal would correſpond with it—ſince the figure it is known by is the only indication of its true nature: but a variation in its external figure has not been inſiſted upon.

There is evidently a contradiction either in the nature of the caſe, or in John Hunter's report of it. If the organs of generation in the free martin be diſſimilar, the figure of every free martin will vary according to the relative predominance of either ſex:—one would have a *penis*, another a *ſcrotum*, with or without teſticles, &c.

whereas externally this is not the fact. I ſhall not pretend to draw any concluſions againſt John Hunter's ſtatement of the appearances from diſſection, becauſe he alone has diſſected them; but I ſhall leave them to be filled up hereafter, when ſome other anatomiſt has made his report upon the appearances of the parts of generation in three free martins which likewiſe *He* has diſſected.

Jan. 17, 1786.

ACCOUNT OF A WOMAN WHO HAD THE SMALL POX DURING PREGNANCY, AND WHO SEEMED TO HAVE COMMUNICATED THE SAME DISEASE TO THE FŒTUS.

PHIL. TRANS.

IF this paper had not been printed by the Royal Society when it was, there would have been no trace of it; as John Hunter did not think proper to reprint it with the reſt of his papers that were publiſhed in the Philoſophical Tranſactions, in his book entitled "Animal œconomy."

Prior to the date of this paper, John Hunter had made up his mind upon theory, and had expreſſed it to his pupils in every courſe of his lectures from their commencement. And when he had made up thus his mind upon theory, no fact ariſing out of practice, be it ever ſo ſtubborn, or poſitive, or true, would induce him to alter his theory:—and this was the reaſon why he has ſaid, that the pregnant woman *ſeemed* only to have communicated the ſmall pox to the fœtus, —and why he has thus obſtinately contended againſt the fact.

His intention was to ſuppreſs this fact as long as he could, from a variety of motives;—for as it was a fact, it accounted for other poſſibili-

ties of infection denied alſo by his theory and his habit of practice :—it accounted for the poſſibility of a fœtus contracting the venereal diſeaſe from the infected mother—it accounted for the poſſibility of a child contracting it from ſucking an infected mother,—and moreover than all this,—it accounted for what has been proved by caſes alſo, the poſſibility of a perſon being infected with the venereal diſeaſe by a tooth being tranſplanted from an infected ſubject into the mouth of a ſound one.

Many of theſe facts having been brought forward, ſince this paper has *ſeemed* to diſpute them, has been the cauſe of his ſuppreſſion of it in his Animal œconomy, and alſo of his dropping the practice of tranſplanting teeth.

I ſhall not have occaſion to argue this queſtion.—I ſhall produce facts againſt his theory; and that which would not ſatisfy his belief, will be proof to all the world beſides,—not excepting his admirers.—As he has contended the caſes given by Van Swieten, and the caſe by Grant, of Mrs. Ford, in *Phil. Tranſ.* vol. 70,—I ſhall refer to one more given in the *Phil. Tranſ.* vol. 71,—and produce the copy of another, the ſtrongeſt in point of fact that can be adduced, and which no contrary theory can hereafter deſtroy.

ſtroy. This caſe, becauſe it militated againſt his theory, John Hunter refuſed to preſent to the Royal Society. However it obtained a reading in 1786.

CASE BY WILLIAM LYNN, SENIOR SURGEON TO THE WESTMINSTER INFIRMARY.

In November, 1785, the wife of Mr. Eve, a coachmaker in Oxford-ſtreet, being then in the eighth month of her pregnancy, was ſeized with rigors, pain in the back, and other febrile ſymptoms. In two days time, the diſeaſe ſhewed itſelf to be the ſmall-pox; and though the puſtules were of the diſtinct ſort, yet they were uncommonly numerous. On the eleventh day they began to turn; and on the twenty-ſecond day her labour took place, which, according to her reckoning, was a fortnight before the regular period; that is, when ſhe was advanced in her pregnancy eight months and two weeks.

The child, at the time of its birth, was covered with diſtinct puſtules all over its body: they did not appear to be full of matter till three days after; at which time I took ſome of the pus upon a lancet, from one of the puſtules on the face. With this lancet I afterwards inoculated, on the 2d of December, 1785, a child of Mr.

Mr. Chaters, in Church-ſtreet, Soho, in both arms. On the 7th, the inflammation began to appear in each arm, and continued daily encreaſing till the 11th of December, when the child ſickened, and was affected with all the ſymptoms which uſually precede the eruption. On the 12th the ſickneſs and fever abated, the puſtules of the diſtinct ſort of ſmall-pox made their appearance, and the child having regularly gone through the ſeveral ſtages of the diſtemper, was perfectly well in three weeks.

It may be proper to obſerve, that Mr. Findlay, ſurgeon, in Sackville-ſtreet, and Mr. Holladay, late ſurgeon to Sir Edward Hughes, in the Eaſt-Indies, were preſent, both at the taking of the matter, and at the ſubſequent inoculation of the child.

As no circumſtance can prove the identity of the ſmall-pox more indiſputably, than its being communicated, with the uſual ſymptoms and progreſſion of the diſeaſe, from one ſubject to another; ſo it appears to be aſcertained from the above facts, that a child can receive the variolous infection from its mother, *in utero*.

June 1, 1780.

ACCOUNT OF AN EXTRAORDINARY PHEASANT.
PHIL. TRANS.

PITCAIRN, having received as a preſent of game, a hen pheaſant, whoſe *feathers* were *variegated* in an extraordinary manner, from a Baronet, exhibited it as a curioſity to Banks and Solander; and John Hunter, happening to be preſent, was deſired to examine the bird, and it proved to be a hen.

Lady T—— had alſo a favorite pyed pea-hen, which had produced chickens eight ſeveral times!!!

Have I not been ſufficiently full upon this ſubject?

AC-

Nov. 14, 1782.

ACCOUNT OF THE ORGAN OF HEARING IN FISHES.
PHIL. TRANS.

NOTHING can more inconteſtably prove the neceſſity there is for every one who undertakes the writing of a life to enquire into *Motives*, than the evidence which this paper illuſtrates. John Hunter begins it, dated in the year 1782, with aſſerting, that the *organ of hearing in fiſhes* is ſtill a ſubject of diſpute, whether they poſſeſs the ſenſe or not. And to prove that he was the diſcoverer of this faculty in fiſhes—he refers back, not to any publication in the year 1760, but to an obſervation made by himſelf at that time, when he was with the army at *Belliſle*.—There, ſays he, I had diſcovered this organ in fiſhes, and had the parts expoſed and preſerved in ſpirits. But leſt his anatomical proof ſhould have deceived him, he tried an experiment inferior in its nature, and more liable to fallacy, than his anatomical diſcovery.—Obſerving in a pond full of fiſh, that they were playing upon the water, when he was at *Liſbon* with the army in the year 1762, he fired off a gun:—the moment the report was made, the fiſh ſeemed to be all of one mind, for they vaniſhed inſtantly, and raiſed a cloud of mud

mud from the bottom. This was not ſo certain and infallible an experiment as that made at *Belliſle*; as if fiſhes poſſeſſed no organs of hearing, concuſſion of air upon water, produced by the exploſion of a gun, might have operated upon their ſenſations.

I ſhall not make any remark upon the probability of theſe obſervations by John Hunter: at any rate they were but obſervations only; and as ſuch, he has, it is true, been ſo far uniform, as to be found in this inſtance, as well as almoſt in every other,—where a contention for making any diſcovery has rendered ſuch a reſource neceſſary for him,—to *antidate* his obſervations, and to *poſtpone* his publications.—He made his diſcoveries in the year 1760, and publiſhed them in the year 1782.

But as the beſt reply to his claim of originality, I ſhall proceed to the demonſtration of facts tending to prove, that he purſued this ſubject not in common even with others, but much later, and that the diſcovery of the faculty of hearing in fiſhes was made prior to any part which he could lay claim to.

I ſhall not lay any ſtreſs of proof from what Willoughby had diſcovered in his natural hiſ-

tory of fiſhes, publiſhed at *Oxford* in the year 1686, as whatever John Hunter had or might have taken from him, he could find in Geoffroy.

I ſhall firſt take notice,—that the Abbé Nollet firſt of all evinced, by diving under water for this purpoſe, as appears in Memoires of Academy of Sciences 1743,—that water was capable of receiving and tranſmitting to the animals contained in it thoſe-particular impreſſions that conſtitute ſound.

Curious figures of the auditory ducts, and of the great variety of little bones or *lapilli*, are exhibited in Klein's hiſtory of fiſh, *Philos. Tranſact. abr.* vol. 9. p. 114.

I ſhall next ſay, that Profeſſor Camper, the great rival of John Hunter, has ſhewn that fiſhes are really endowed with the faculty of hearing, and has deſcribed the anatomical organs adapted to this purpoſe in a diſſertation publiſhed by him in November 17, 1761, and again in 1774. The following are two of the titles which may be produced from a *ſyllabus* of all the works of this accompliſhed ſcholar and ingenious philoſopher, —beſides *three* more, which I have not noticed.

Harlem: 1762, Tom. 7. pars 1. *Diſſertatio de Organo Auditus Piſcium Squammigerorum, p.* 79, *quod* 17 *Nov.* 1761, *primus detexi.*

Pariſiis

Parisiis, 1767, in actis, &c. *Memoires de Mathematique, et de Physique, presentés a l'Acad. R. des Sciences, &c.* 1764. *Tom.* 6, *p.* 177, reperitur dissertatio, seu *memoire sur l'origine de l'oüie des poissons.* De organo auditus piscium, gallicé: descripsi in eâ organum auditus, et cerebrum *Lophii*, seu *Ranae Piscatricis*, *Esocis*, et *Rajae.*

And lastly, I shall say,—that the commissaries of the Royal Society of Physicians at *Paris*, appointed to examine the work of M. Geoffroy, entitled *Dissertations sur l'Organe de l'ouie de l'Homme, des Reptiles, des Poissons*, published in 1778, infer,—that the human species, quadrupeds, and cetacious fishes, form the, *first*, class of animals whose organ of hearing is, the most subtle, and of the most perfect construction;—that birds are to be placed, in the *second*;—reptiles, who have the external mark of the *tympanum*, in the *third*;—cartilaginous fishes, in the *fourth*;—spinous or prickly fishes, in the *fifth*;—fishes of the eel kind, which seem to have only two semicircular canals completely formed, in the *sixth*;—and serpents, who appear to have no semicircular canals, in the *seventh.*

Do not these facts which I have adduced, demonstrate the natural propensity, or imbecility

rather, in John Hunter, ſtronger than any words of mine can prove it? What of originality had he to boaſt in the diſcovery, of hearing in fiſhes, by a publication ſo late as the year 1782, when he had only, as it plainly appears, the gleanings from others to pick up?

1785.

ACCOUNT OF A NEW MARINE ANIMAL.

PHIL. TRANS.

I AM at a loſs to account for this animal, or rather ſpecies of worm, being deſcribed as *new*. Whatever novelty there is annexed to its nature is to be conſidered as that peculiar ſort, which is naturally a reſult of the firſt impreſſion made upon the ſenſes, and which is the effect of an individual never having obſerved the ſame object before. But if this animal is to be called a *new* one from no one having ever diſcovered or previouſly deſcribed it, I muſt beg leave to oppoſe thoſe who might have ever entertained ſuch an idea.

Amongſt the rocks between the iſlands of *Nevis* and *St. Chriſtopher's*, the greateſt varieties are to be found, lodging themſelves in little cavities of thoſe rocks and of the larger ſea plants of the ſtony kind. They are diſcovered by the branching forth of their *Tentacula*, which exhibit a beautiful diſplay of various colours, reſembling a fine double flower.

A gentleman who had a place in the cuſtom houſe at the iſland of *Nevis*, and who had a taſte

taſte for natural curioſities, exhibited an extraordinary collection of American *Actiniæ*; and in this collection no leſs than five of this ſpecies, here termed a *new* animal, were to be ſeen: their appearance in the body, when drawn out, and their ſize, reſembling a leech all but in colour. But after all, it might perhaps have been never ſeen before at Barbadoes, from whence this deſcription was ſent; and the revolution of the coaſt produced by the *Hurricane*, might have given a new creation to this ſpecies of *Actinia*, never found by any one at *Barbadoes* till then. Why had it not occurred to the correſpondent who ſent the account to John Hunter,—that the *Hurricane*, which he has deſcribed to have been previous to this new birth in the creation, might have been the *groanings of nature* in the very act of parturition?

In the Linnæan ſyſtem, this is of the *molluſca* order of worms, including five ſpecies. Of this, in the Philoſophical Tranſactions, John Hunter has given two ſorry plates, and out of proportion:—one deſcribing the animal in its ſtony ſhell, with its double *Tentacula* ſpread out of it—and the other the body of the animal itſelf out of its ſtony ſhell.

A TREA-

March 1786.

A TREATISE ON THE VENEREAL DISEASE.
4to. 1l. 1s.

THIS work of the celebrated author has already engaged my attention, and undergone my criticism through three publications following each other, from June 1786 to their completion in May 1787—comprising in the whole 465 pages 8vo.

If in these I have not said enough upon the subject, and if what has been said by me be not well said, nothing which I now could advance will avail me any thing. But I have not the smallest ambition—for reprinting or circulating any opinions which I have laboured to inculcate, beyond that becoming point of a fair publication;—for fanning the breath of fame by sending forth old publications under new titles,—and for making up a new book on "animal œconomy" out of old papers in the Philosophical Transactions. Nor should I have noted this now of myself, or ventured to have spoken at all of myself, but by way of apology for a chasm here apparent,—a chasm of no less magnitude in the professional life of John Hunter, than will be naturally

rally ſeen from his great work on the *Venereal Diſeaſe* being thus omitted.

But there is an obſervation which readily offers to ſome:—that I, who attempted to think for myſelf in his life time, and who then examined ſome of his productions, am more juſtified on thoſe I am now engaged in, than another would be, who waited when John Hunter could not defend himſelf. But even that argument, in my opinion, is only applicable to the weaker ſpecies of men; and they ſhould be told—that it is his works which I am conſidering;—that theſe refer to life beyond the limit and the power of him who produced them;—that theſe are bequeſts from him of the art of ſurgery and of phyſiology to poſterity—and are at any future time liable to be examined, although with leſs means for the obtainment of truth;—as the further any one is removed from an object, the more obſcurely will its reality be diſcerned.

Independant of what the opinions of intelligent enquirers after knowledge have done for us both in this country, I ſhall, without a doubt upon the queſtion, produce a tranſlation of the *Review* which the Univerſity of *Gottingen* has given between us, on the ſubject of the Venereal Diſeaſe, with only remarking, that it has been

ſent

ſent to me ſince it was known I was thus engaged.

(*Tranſlation.*) *Gottingen* 1787, vol. 3. p. 1922. " J. Hunter's Diſſertation on the Venereal Diſeaſe, from the Engliſh, with three plates, 1787. Pages 688, in great 8vo. excluſive the regiſter.

" There are not ſo many annotations and moſt neceſſary informations as in page 339 of the knowing and learned tranſlator, as would have been neceſſary for making this book by a tranſlation only, univerſally uſeful, which in his original contains ſo many ſingularities and numberleſs errors. It is to be wiſhed that the tranſlator had, as in page 140, uſed through the whole book the admonitions which *J. Foot* has made againſt it. In this caſe the tranſlation would have had a very great preference of the original itſelf."

Such was too proud a teſtimony when coming forth from that univerſity, nurſed into eminence by the illuſtrious de Haller—by him who firſt filled the chair of preſident, and which he vacated only with his breath—from that univerſity where modern literature looks for meritorious patronage. Let not the admirers of John Hunter aſſume a ſottiſh ſcorn and frown indignantly

on *Truth* becauſe her ſmiles have been thus beſtowed upon me :—ſhe will but the more from that cauſe careſs me. And if ſhe come delegated to me by a *German* univerſity before ſhe has been ſent to me by my countrymen, I am content :—I am but in that predicament of a greater man, and whom I only can imitate in my ſincerity for the obtainment of knowledge.—Lord Bacon has ſaid, " that the preſent juſtification of my name I leave to foreign nations, and the future to my countrymen after ſome time has paſſed over." I do aſſure John Hunter's admirers that I never was but once in Hanover, and not then at Gottingen, and that I have no influence perſonally there ;—but if I ever viſit Switzerland, my zeal would direct me to Berne, where de Haller lies entombed, and would lead me to follow the example of Tully, who ſought through brambles the tomb of Archimedes.

March 22, 1787.

AN EXPERIMENT TO DETERMINE THE EFFECT OF EXTIRPATING ONE OVARIUM UPON THE NUMBER OF YOUNG PRODUCED.

PHIL. TRANS.

THE practice of ſpaying or ſpading animals, is vulgarly known to the loweſt order of men, and it is generally followed by the idleſt man in every pariſh.

When this operation is performed effectually, the two *ovaria* which are appointed by nature to every animal, are taken out. The uſual way is to make the inciſion in the middle of the flank, rather aſlope, two inches and a half long, that the fore finger may reach the *ovaria*, which are two ſubſtances in a female dog about the ſize of an *acorn*, on both ſides of the *uterus*; one of which is firſt drawn out of the wound and ſeparated from its connection,—and the other then taken out alſo.

The purpoſe of John Hunter's experiment was to take away from a Sow *one ovarium*, and leave the other,—in order to aſcertain how far this partial privation would influence the animal reſpecting the quantity of young which ſhe might hereafter produce, in proportion to ano-

ther Sow that had not been thus partially ſpayed; —both Sows being in every other ſenſe as nearly ſimilar as poſſible.

I ſhall not make any comment upon his introduction to this ceremonious piece of curioſity; as it would be endleſs to follow up with remarks the obſervations of him who thinks and writes at random,—who never ſeriouſly weighs what he has to offer, and never ſtands in awe of an abſurdity. He has heard of there being ſuch a thing in reality as a *third* in its nature diſſimilar to the *two* which has produced it, and this he has applied to the young of a female. However this *Tertium* of our philoſopher is generally of a ſimilar nature to *one* or *other* of its parents.

The reſult of this experiment was, that the ſpayed ſow ceaſed to breed two years before the other.—That the ſpayed ſow produced in all her farrows 76 young in four years—and that the perfect ſow produced 162 in ſix years.

John Hunter infers from this experiment—that the *ovaria* are from the beginning deſtined to produce a fixed number, beyond which they cannot go, although circumſtances may tend to diminiſh that number. He has obviated every poſſible objection to this experiment by a note, where he ſays,—"It might be thought by ſome, that

that I ſhould have repeated this experiment; but an annual expence of twenty pounds for ten years, and the neceſſary attention to make the experiment complete, will be a ſufficient reaſon for my not having done it."

I do not know that ſuch is the deſtination of the *ovaria*; for if it were, the general conſequence would be, that with few exceptions, the number of young from every ſow would be nearly equal:—but whether this be the fact or not—whether every ſow nearly produces any equal quantity of young or not—this experiment wanders from aſcertaining *that*, much more than common obſervation would, upon the quantity of young by each produced, when made by a ſow-breeder. If he had neither time nor diſpoſition for the expence attending the whole which was neceſſary for the perfecting of his experiment, that was his own concern: the public has nothing to do with it:—the public, if they wanted any part of this experiment, wanted what was ſatisfactory for aſcertaining the preſumed fact; but being diſappointed, it was not for him, nor was it the part of a phyſiologiſt, to ſet the minds of men a gadding after a *Chimera*.

John Hunter ſhould have been told,—that nature does nothing in vain, and that ſupererogation

gation is not to be found in her œconomy. Either the *ovaria* like the *teſtes* are amongſt her ſecurities for making procreation more certain, when one or other of them might be rendered from diſeaſe imperfect,—or both are neceſſary for the complete purpoſes of procreation. I am apt to conclude that the two *ovaria* are for the purpoſes of ſecuring procreation, and not as he ſuppoſes for encreaſing it. But whether the truth lies with his opinion or mine, nobody will think of ſearching for it; as whoever keeps breeding ſows, or any other animals for breeding, is generally deſirous that they ſhould be prolific, and therefore any means which may be deviſed for reſtraining procreation *partially*, will be conſidered as an officious and thankleſs kind of information.

April, 1787.

OBSERVATIONS TENDING TO SHEW THAT THE WOLF, JACKAL, AND DOG, ARE ALL OF THE SAME SPECIES.
PHIL. TRANS.

IT will be neceſſary firſt of all to ſtate—that two different animals may breed, but that the animals produced by them, may be incapable of going on with further procreation. Thus from a mare and a jack aſs, a mule is gotten, but the mule is not known to breed—or at leaſt the fact is ſo rare, as to be diſputed. The race therefore of mules would be extinct, if it depended upon any power in them for propagating a ſpecies.

But the facts adduced in this paper tend to prove,—that the wolf, jackal, and dog, not only copulate with each other and produce young,—but that their young can alſo copulate and produce young, to ſucceeding generations. It has been proved thus far in part, but not altogether.—It has been proved that a dog and a wolf will produce young; and which young will go on with propagation;—and it has been thus proved by a dog and a jackal, but not by a wolf and a jackal—therefore John Hunter's title is erroneous.

Lord

Lord Pembroke had a bitch-wolf half bred, from a dog lining a bitch-wolf—ſhe produced puppies by being lined by a dog:—and a bitch from this litter produced four litters of puppies, by four diſtinct dogs. This bitch lived twelve years, was buried in his garden—and over the place of her grave was written the following inſcription:

Here lies *Lupa*,
whoſe grand-mother was a wolf, whoſe
father and grand-father were dogs,
and whoſe mother was half wolf and half dog.
She died the 16th of October, M.DCC.LXXXII,
aged 12 years.

John Hunter procured a bitch jackal, half bred from a dog lining a bitch jackal, and this bitch was lined by a dog and had puppies. Such is the ſummary of this paper.

There is yet a palpable imperfection which ought to have been obviated. It does only appear, that the breed was carried on by dogs copulating with the half bred bitch-wolf, and bitch jackal: whereas it was neceſſary for perfecting the ſyſtem, that the litters from them ſhould have interlined and produced their breed;—or that

that males of their own litter, ſhould have lined the bitch half bred wolf, and jackal.

John Hunter has ſaid,—that the fox ſeems to be farther removed from the dog, than either the jackal or the wolf,—and that it is confidently *aſſerted only*, that the dog and the fox will breed, but that this has not been aſcertained. Such is the refined taſte of this phyſiologiſt,—that he doubts obvious facts, and eſtabliſhes difficult ones. There is not a fact more generally received, more obviouſly to be aſcertained by every enquirer into natural hiſtory,—than the known power of propagation between the dog and the fox,—and alſo, than the known power in their offspring for *continuing on* that propagation. To thoſe who knew him as well as I did, the cauſe of this feigned ſcepticiſm will be eaſily traced. He wanted to pluck a feather from the cap of Buffon, who has expreſſed himſelf with delicacy upon this ſubject: but yet at length, from the fact of a dog and wolf having copulated and produced young, he is free to admit,—that they not only are of one *genus*, but nearly of the ſame *ſpecies*:—and this in my opinion is all that ought to be granted—even from the freſh facts which have been adduced in the paper which I am now conſidering.

John Hunter could not prove—that the dog and fox were not only of the ſame *genus*, but *ſpecies* alſo, by a ſpontaneous unreſtrained copulation of two ſorts of animals exiſting in common in the ſame country; and therefore he denied their alliance. But I will go farther,—I will defy any man to prove—that herds of wolves and jackals would forſake their *proper* kind, and *ſpontaneouſly* copulate with dogs. The queſtion taken in a liberal ſenſe is not—what is ſeen to be done by a compulſion in the calls of nature,—but what would be done, by the general free accordance of it;—and in this view, Buffon has neither declared too much nor too little; but has confined his obſervation ſtrictly within the limits of propriety.

It appears from Buffon, that he did not ſucceed after many attempts, to get his bitch wolf to copulate with a dog—but that, although the experiment failed with his, it ſucceeded with another's.—It appears that the bitch wolf, which was the origin of the Produce belonging to Lord Pembroke, was ſolitary and confined by Brooks of the *New Road*, and therefore, when ſhe was in *heat*, thus *ſubmitted* to copulation with a dog.—And it appears, that the bitch jackal was on board of a ſhip, when ſhe was lined by a dog, of which

which the whelp that John Hunter had, was the Produce.

Do theſe facts give nature the fair play to found new reaſoning upon? And is the natural boundary of her laws, by which ſhe is governed, to be thus ſophiſtically miſconſtrued?

1787.

OBSERVATIONS ON THE STRUCTURE AND ŒCONOMY OF WHALES.

PHIL. TRANS.

FROM the very nature of this ſubject,—from the magnitude of the animal, and the intereſt it bears to ſociety by the commerce of oil, ſperma-ceti, and whalebone,—from the tons of ſhipping and the body of ſailors, that it keeps in employment,—and from the enquiries in conſequence, which go forth, more eſpecially amongſt thoſe who have the adminiſtration of public affairs, and commercial regulations—men of education, activity and buſineſs,—it is morally impoſſible, but that ſomething ſhould have been known about Whales before John Hunter preſented this paper:—if all intercourſe in natural hiſtory had been excluded from the inhabitants of Great Britain, of what was paſſing in other parts of Europe, the very ſpirit of trade in order to find out—how much more could have been made of a whale—would have commanded an attention to the ſubject—if any information had been thought defective, *even* by a *premium* for obtaining it.

I have peruſed this paper with more than common attention. I was induced to it, from the

the vaſt promiſe I had heard of it,—from what the newſpapers of the different times had announced—as he never did any thing of this ſort without having the kindneſs to acquaint the public—and from what his admirers had alſo ſaid about it.

But—it is to be preſumed at any rate,—that John Hunter could never have poſſibly given the Linnæan deſcription of the *genera* and *ſpecies* of whales without having read; and having read upon the ſubject,—it was to have been preſumed —that he would have had the goodneſs to have told us in this paper, who were the authors he had read, and from which of them he had taken his *claſſic Nomenclature*;—as in all probability where he found *that*, he found more. But it has not pleaſed him to quote a ſingle name excepting that of Dale, the author of Antiquities of Harwich.

Such has been the conſtant uniform practice of this modern philoſopher; and he would not have given the claſſic names, but for the pomp which accompanies his introduction; as after he has once copied them, he drops them. He leaves the *Phocæna*, for Porpoiſe, and the *Delphinus Delphis*, for Bottle noſe. He never after is found to uſe any other than the vulgar terms, in which he

he was at home at his eaſe; and with theſe he proceeds in his enquiries

A Whale in the Linnæan ſyſtem of Zoology, is the ſeventh order of the claſs of *Mammalia*; the characters of which are,—that the animals of this order, have breathing apertures on the head, —pectoral fins,—the tail placed horizontally,— and no claws. The order includes four *genera*, viz. *Monodon* or ſea Unicorn, *Balæna* or whale, *Phyſeter*; and *Delphinus*, comprehending the Dolphin, Porpoiſe and *Grampus*. From theſe four genera, the various ſpecies are claſſed by this accurate phyſiologiſt.

And although the enquiry cannot be purſued into the ſtructure and œconomy of whales, which any other author has made, by referring to this paper of John Hunter,—although he has followed his natural propenſity, by ſhutting out every piece of relative information upon a ſubject thus curious, valuable and important,—yet it will be found to have engaged the attention of naturaliſts, and anatomiſts from very remote times, down to the preſent. Thus far I can aſſert, that the *genera* are deſcribed by terms of Greek,— and that Ariſtotle and Pliny have both deſcribed the whale:—that the ſubject has been treated on in Pantopidan's Natural Hiſtory of Norway, —in

—in Crantz's Hiſtory of Greenland,—in Pennant's Zoology,—and that there is not a ſingle modern German anatomiſt, who has omitted to diſſect and deſcribe the *Balæna*.

John Hunter in this paper ſays, that he examined the *Delphinus Phocæna* or Porpoiſe, both male and female, ſeveral of them.—The *Grampus*, two of them.—The *Delphinus Delphis*, or Bottle noſe, a young and an older one.—The *Balæna Roſtrata* of Fabricius, one, which was ſeventeen feet long.—The *Balæna Myſticetus*, or large whalebone whale, the *Phyſeter Macrocephalus*, or ſpermaceti whale, and the *Monodon Monoceros*, or Norwhale, have alſo fallen under his inſpection. Some of theſe were too long kept before he procured them, to admit of more than a very ſuperficial inſpection.

By this account it appears,—that he had examined ſeveral of the moſt trifling ſpecies, ſuch as the Porpoiſe and Grampus,—that the largeſt was the *Balæna Roſtrata* of Fabricius, only ſeventeen feet,—and that the moſt important and valuable ſpecies, viz. the *Balæna Myſticetus*, or large whalebone whale, the *Phyſeter Macrocephalus*, or ſpermaceti whale, and the *Monodon*, or Norwhale, had only "fallen under his inſpection." I have every reaſon to believe, that he never ſaw

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in his life time, a *Balæna Myſticetus* and a *Phyſeter* awhole, either dead or alive :—as theſe are ſo very important in point of ſize, and of whalebone, oil, and ſpermaceti ;—as theſe are found ſo remote from England ;—as theſe never have been, nor can be brought home awhole ;—as the value of theſe, conſiſts in what is taken from them ;—the world would have wrung with his report of himſelf upon ſuch an occaſion,—if he had really diſſected ſuch whales,—inſtead of its being but barely told in the fainteſt tones of their " having fallen under his inſpection." It is for thoſe reaſons, I am almoſt perſuaded that he neither had diſſected nor ſeen them.

In this paper, He is particularly diffuſe, and opens his ſubject upon general obſervations—that the anatomical appearances of various whales are not uniform,—that notwithſtanding whales have urinary bladders, there is no apparent reaſon why they ſhould have them. He goes into a chemical diſquiſition upon Spermaceti, a ſcience with which he was totally unacquainted ; and he has aſſerted, of which I truſt merchants will reap the advantage, a novelty,—that Spermaceti is not confined to the head only of a whale, but is to be found every where. He diſcuſſes his ſubject under different heads ; and in treating on the mouth, the following obſervation will ſhew what

was

was the radical knowledge he poſſeſſed of the ſpecies of whales.

"There is a very great variety in the formation of the mouths of this tribe of animals, which we have many opportunities of knowing, from the head being often brought home, when the other parts of the animal are rejected; a circumſtance which frequently leaves us *ignorant* of the particular ſpecies to which they belonged."

Perhaps the hiſtory of criticiſm never furniſhed a ſtronger inſtance of its neceſſary utility, than is exemplified by the above paragraph: as every definition of the varieties in the ſpecies of whales, is principally if not wholly dependant on the forms of the head,—of the teeth—of their having ſome or none, few or many—of their tongues, their noſtrils, their ſpermaceti, their whalebone—with a wonderful variety of other certain characteriſtics, which cannot eſcape a converſant phyſiologiſt.

He proceeds to deſcribe the whale bone in the jaws, and proves it to be an animal ſubſtance not bony, but ſimilar to hoofs, hair, nails, and feathers. He ſays that the mouth and *œſophagus* are wider than in other animals, but not in proportion to the ſize of the fiſh;—that the ſtomachs in each are in number from five to ſeven;—that

their food is fiſh; that their inteſtines are uniform,—and that they have kidnies, ureters and bladder—that their kidnies are made up of ſubſtances put together like a pavement.

The blood of whales is in great profuſion, the heart large, and the arteries reſemble thoſe of other animals. He deſcribes the *larynx* and the lungs—that they conſiſt of two oblong bodies, are very elaſtic, and have very ſmall cells;—He alſo demonſtrates a diaphragm.

In his deſcription of what he calls the blow hole, or paſſage for the air, he ſays, none but the whale-bone whales have the organs of ſmelling: the Porpoiſe, Grampus, Bottle noſe, and Spermaceti whales have but one orifice externally: whale-bone whales have a double. The *glottis* and *epiglottis* are united with the poſterior noſtril, ſo as to ſhut water out from the lungs. They have both *cerebrum* and *cerebellum*.

Whales, he ſays, poſſeſs the ſenſe of touch, which is ſeated in their cuticle; they have tongues which vary in the different ſpecies, and theſe are endowed with the ſenſe of taſte. He is extremely diffuſe upon the organ of hearing, and has diſplayed a wonderful degree of accuracy upon that ſubject. This led me to ſuſpect that he had ſome

ſome deſign by being thus particular, more than on any other property of the whale: I attended through the whole of his deſcription, but could not find a ſingle authority quoted by him: but on turning to the *ſyllabus* of Petrus Camper's works, I found—that the Profeſſor,—John Hunter's formidable rival,—had treated on this very ſubject in the year 1765 and 1776.

Harlemi. Act. Harlem. Tom. 11. *part.* 3. *De Organo Auditus Ceti. page* 193. *anno* 1765.

Ib. Tom. 17. *part* 1. *De Sede Organi Auditus, ejusque præcipua Parte Oſſea in Balænis Myſticetis, egi* 1776.

Are not John Hunter's admirers obliged to me for demonſtrating to them theſe facts;—and am not I more ſincerely their friend, than he was, who would not impart to them, even in confidence, what I ſo readily do?

The future part of this paper is huddled up in a very ſummary manner; and where much was expected—where curioſity was moſt excited, the leaſt information is obtained. This paper is of ſome length in the Philoſophical Tranſactions—is ſwelled by his abſtract reaſonings,—and deficient in matter of fact. It is that very ſort of

paper, that a man would write upon a ſubject which he does not underſtand. When Johnſon had read Cibber's Hiſtory of the Stage, he gave that performance the beſt of all characters by the following obſervation upon it—"See how eaſy it is for a man to write upon a ſubject which he well underſtands!!"

He has deſcribed the parts of generation—the teſticles of the male ſituated within the *abdomen*: he has not mentioned any thing of their ſize. He has alſo deſcribed the *penis* under the ſame cloud of obſcurity. Of the female parts of generation, he ſays, that the external opening is a longitudinal ſlit—that the reſt conſiſt of *vagina*, two horns of the *uterus*, fallopian tubes, *fimbriæ* and *ovaria*,—and that theſe throughout are found to be uniform.

There are annexed to this performance no leſs than eight plates of whales—and parts of whales.

He ſays, he does not know, when they copulate, whether they do it in an erect poſture, or otherwiſe; nor any thing of their time of geſtation. The female has two nipples on the poſterior part of the *abdomen*, and he thinks ſhe brings forth but one young at her time of parturition.

OBSER-

OBSERVATION.

The article of April 30, 1789—Phil. Trans. "*A Supplementary Letter on the Identity of the Species of the Dog, Wolf, and Jackal,*" being annexed to that of April 26, 1787, being printed with it in his Animal œconomy, and making there, but a continuation of the ſame paper,—has been already reviewed with that paper. *The ſix Krohnian lectures* on Muſcular Motion, from 1776 to 1782, are not publiſhed in the Philoſophical Tranſactions, but they have been printed and partially circulated among the Fellows,—and I have peruſed them. That which does not face the public eye, I have no deſire publicly to inveſtigate,—and therefore I decline it, from that cauſe alone.

OBSER-

1792.

OBSERVATIONS ON BEES.
PHIL. TRANS. PART I. P. 128.

I SHALL firſt of all, give a ſhort abſtract of the contents of this paper, and then my comment ſhall follow. John Hunter opens his ſubject with the hiſtory of the bee, ſaying that the bee has been rather conſidered as a fit ſubject for the curious at large; whence more has been conceived than obſerved :—that Swammerdam indeed has erred on the other ſide, having with *great induſtry* been *very minute* in the particular ſtructure of the bee; and again he ſays, that Swammerdam often attempted *too much accuracy* in his deſcription of minute things. He finds bees to be the inhabitants of Europe, Aſia, Africa, and America; and he proceeds to deſcribe the general properties of the female or working bees, telling us that accidents happen to bees, and that his bees have been ſubject to them. He aſcertains the heat of bees, and the heat which eggs require. He has found out that bees never evacuate their excrement in the hive: but he has not told us, what they do in the winter, when during ſeveral months, they never quit the hive. He talks about their ſociety, and ſays, when the queen is loſt, their attachment

ment is broke; they give up induſtry; probably die; or we may ſuppoſe join ſome other hive.

The ſwarm he makes to conſiſt of three claſſes —a female or females, males, and thoſe commonly called, mules, which he ſays are ſuppoſed to be of no ſex, and are labourers. A hive ſends off two or three ſwarms in the ſummer: the ſwarm ſometimes goes back, he believes from the loſs of their queen: he killed ſeveral of thoſe that came away, and found their crops full, while thoſe that remained in the hive had theirs not ſo full. Having thus as he ſtiles it, ſet the bees in motion, he proceeds to the ſubject of

WAX.

This is the material of their dwelling or comb: and he fully affirms—*that it is their wax*: and his next conſideration is the mode of forming, preparing, or diſpoſing of it, and in giving—*a totally new account of the wax.*

I ſhall firſt ſhew, ſays he, it can hardly be, what it has been ſuppoſed to be. I have obſerved, that when the weather has either been ſo cold, or ſo wet, as in June, as to hinder a young ſwarm from going abroad, they have yet

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in that time formed as much new comb, as they did in the ſame time, when the weather was ſuch as allowed them to go abroad. *The wax is formed by the bees themſelves. It may be called an external ſecretion of oil, and I have found,* ſays he, *that it is formed between each ſcale of the under ſide of the belly.* In the bottom of the hive he ſhews us a good many of the ſcales lying looſe, and ſome pretty perfect, others in pieces. I have endeavoured, ſays he, to catch them either taking this matter out of themſelves from between the ſcales of the abdomen, or from one another, but *never could ſatisfy myſelf.* It is, he alſo adds, with *theſe ſcales that they form the cells called the comb*; but perhaps not entirely, for I believe they *mix farina with it*; however this only occaſionally, when probably the ſecretion is not in great plenty. The bees, he ſays, who gather the *farina*, alſo form the wax, for I found it between their ſcales.

After explaining this diſcovery, he proceeds next to deſcribe the comb and its cells,—then the laying of the eggs—from the eggs to the maggots with their food—and from the maggots to the chryſales, with their coats—making the egg in hatching five days, the age of the maggot four, and the chryſalis thirteen: but he adds how far it is accurate, he cannot ſay.

He

He treats of the ſeaſons, when the different operations of the bees take place. He ſays—in the month of Auguſt we may ſuppoſe the queen impregnated by the males; and as the males do not provide for themſelves, they become burdenſome to the workers, and are therefore teazed to death much ſooner than they would otherwiſe die. And when the bees ſet about this buſineſs of providing the winter ſtore, every operation is over, except the collecting of honey and bee bread. The whole of the males are now deſtroyed; and indeed it would have been uſeleſs to have ſaved any to impregnate *the queen in the ſpring*. In the winter months they live on the produce of the ſummer, and get as cloſe together as the comb will let them. In this manner they appear to live through the winter. He ſtates the conſumption of ſomething within the hive during the winter, by the difference of its weight being ſeventy two ounces and ſome drams leſs,—looſing from month to month more or leſs of that balance in the end.

He next proceeds to the proceſs of the *Queen Bee* for incubation, in conſequence of her having been *impregnated by the males in Auguſt*, that is, *ſix months to March following*, when he ſays the eggs in the oviducts are beginning to ſwell: —and he farther ſays,—I believe in the month

of March ſhe is ready to lay them, for the young bees are to ſwarm in June, which conſtitutes the queen bee to be the earlieſt breeder of any inſect we know. He ſays,—he found, in April, young bees in all ſtages.

He proceeds upon the queen bee, and tells us—that ſhe has excited more curioſity than all the others, although much more belongs to the labourers. After having conſumed ſix pages in criticiſms upon the late diſcoveries of Schirach, and the opinion of Wilhelmi, which will be explained by me in proper time, he ſays,—that the queen—in whatever way produced—is a true female, and different both from the labourers and the male. He deſcribes her, and adds, it is moſt probable that the queen which goes off with the ſwarm is the young one; for the males go off with the ſwarm to impregnate her, as *ſhe muſt be impregnated the ſame year becauſe ſhe breeds the ſame year.* The queen, he adds, has a ſting ſimilar to the working bee.

He believes a hive or ſwarm has but one queen, at leaſt he never found more than one in a ſwarm. Supernumerary queens are mentioned, he ſays, by Riem, who aſſerts he has ſeen them killed by the labourers, as well as the males. Riem, he ſays, alſo aſſerts *he has ſeen the copulation*

tion between the male and the female, but does not ſay at what ſeaſon. He doubts this.

He proceeds next to ſay,—that Schirach ſuppoſes the queen impregnated without copulation. He knows not whether Schirach means by this that ſhe is not impregnated at all, and ſuppoſes, like Debraw, that the eggs are impregnated after they are laid, by a ſet of ſmall drones, who paſs over the cells and thruſt their tails down into the cells, ſo as to beſmear the egg. He then adds a note to this, ſaying that Debraw, knowing the drones *died in the latter end of ſummer or the autumn*, was obliged to ſuppoſe a ſmall ſet of males that lived through the winter for the purpoſe.

He then ſays—that, the circumſtances relative to the impregnating the queen not being known, great room has been given for conjecture, which if authors had preſented as conjectures only, it would have ſhewn a candour; but they have given what in them were probably conceits, as facts.

He then deſcribes the male bee, which is known by the name of drone. He tells us the males are hatched the lateſt, but alſo that the maggots are too young for the *inveſtigation* of them,—they being all very much of the ſame

ſize. In the month of Auguſt, *probably* about the latter end or beginning of September, they are dying, but *ſeem* to be haſtened to their end by the labourers.

He proceeds to deſcribe the Labouring Bee; of which he ſays,—this claſs, for we cannot call it either ſex or ſpecies, is the largeſt number of the whole community. There are thouſands of them to one queen, and probably ſome hundreds to one male; as, ſays he, we ſhall ſee by and by. It is ſuppoſed they are the only bees which conſtruct the whole hive, and that the queen has no other buſineſs but to lay eggs. They are the only bees that bring in materials; the only ones we obſerve being abroad; and indeed the idea of any other is ridiculous, when we conſider the diſproportion in numbers, as well as the employment of the others, while the working bee has nothing to take off its attention to the buſineſs of the family. They are ſmaller than either the queen or the males: not all of an equal ſize, although the difference is not very great. The queen and the working bees are much alike: they are all females in conſtruction, having the female parts, which are extremely ſmall, and would be eaſily overlooked by a perſon not very well acquainted with the parts in the queen. This has been obſerved by Riem.

Indeed

Indeed one might ſuppoſe that they were only young queens, and that they became queens after a certain age; but this is not the caſe. They have ſtings, which is another thing that makes them ſimilar to the queen. He gives an account of the ſtings—and concludes this article with ſaying that nine thouſand bees will thereabouts fill two quarts.

He then proceeds to the parts concerned in the nouriſhment of the bees. He deſcribes the tongue *very fully*; and next the *œſophagus*, at the end of which there is a fine tranſparent bag, which is the immediate receiver of whatever is ſwallowed. In this bag the bees depoſit their honey, part of which is regurgitated, and the reſt goes into the ſtomach for digeſtion and nutrition: whatever remains to be regurgitated is never found to be altered;—it is pure honey. He proceeds to deſcribe the ſtomach; and when he has finiſhed this ſubject, he begins with the ſenſes of the bees,—then their voice,—then their female parts, and the oviducts of all the common female bees:—and this, with every other anatomical part, is done in a Swammerdam-like manner. He proceeds in the ſame Swammerdam-like manner to deſcribe the male parts, as if he had really the power—had beſtowed the ſame attention—and had as much time for it—as Swammerdam had.

He

He then recurs back to a former ground, and with a miſgiving ſcepticiſm debates his *firſt aſſerted belief of the death of all the males in Auguſt*, eſpecially as thoſe bees he had termed in his firſt outſet to be *mules*, have been ſo lately deſcribed by him, or rather Swammerdam, as all to poſſeſs female parts of generation. He ſpeaks again *of the queen breeding in April by the impregnation of the males who died in the preceding Auguſt*, and ſays,—what is very true,—that this muſt *puzzle* any one not acquainted,—(or acquainted I ſay,)—with the mode of impregnation of the females of moſt inſects. He flies to his uſual reſource of proving his facts relative to one inſect, by experiments made not on that ſpecies, but on another, as if he doubted his own hypotheſis. He makes thoſe experiments on ſilk worms, and endeavours to make the caſe of the bee bend to the caſe of the ſilk worm: and as he flies to experiment for this,—it muſt be preſuppoſed,—that before he began thoſe experiments, he knew *leſs of the ſilk worm* than he did of the bee—as what he has ſaid of the bee, he was told by former authors over and over again: —and as the facts depended upon experiment, it is to be preſumed—that he did not know what he was to learn from the ſilk worm—until his experiment was gone through.

After

After having exemplified to his own ſatisfaction, that he had aſcertained with preciſion, the truth of the *autumnal impregnation* for the *ſpring propagation*, not by an experiment made on bees but on ſilk worms,—he proceeds to deſcribe the ſting of bees anatomically, after the manner of Swammerdam alſo; and from that, he ſports his opinion on the life of bees. He has obſerved—that the life of the male, is only one ſummer, or rather a month or two; and this we know, ſays he, from there being none in the winter; otherwiſe their age could not be aſcertained, as it is impoſſible to learn the age of either the queen, or the labourers. Some ſuppoſe, he ſays, that it is the young bees which ſwarm; and moſt probably it is ſo; but, he adds, I think it is probable alſo,—that a certain number of young ones may be detained to keep up the ſtock. There muſt, he adds, be a period for a bee to live, and if I were to judge from analogy, I ſhould ſay—that a bee's natural life is limited to a certain number of years. Pray can his admirers tell us what is the animal whoſe period is not limited? One bee, he ſays, does not live one year, another two, and another three:—but has not John Hunter already ſaid, what nobody diſputes,—that the drones live but one ſummer, nay not more than two or three months?

After

After this—he proceeds to be more extravagantly opiniative, than in what I have thus lately detailed; and at length reverts to the comb of the hive, which he figuratively calls the bee's *furniture*, and which is wearing out and in time unfit for uſe. He obſerves that the bees did not clean out the excrement of the maggots which croud the cells and fill them up, making them clumſy in compariſon with original ones.

This ſubject is not ſimilar, in its nature, to many of thoſe choſen by John Hunter; it is of univerſal notoriety, and perhaps has excited the enquiries of naturaliſts beyond the reach of our hiſtorical knowledge. Among the antients who have treated on it, are Ariſtomachus, Ariſtotle, and Pliny; and among the moderns who have moſt claimed attention, are Swammerdam, Meraldi, Reaumur, Thorley, White, Wildman, Riem, Schirach, Wilhelmi, Bonnet, and Debraw. Every article which refers to bees, has been fully diſcuſſed by one or other of thoſe authors.

For

For aſcertaining their minuteſt anatomy, and other inveſtigations into bees, Meraldi, Reaumur, and Swammerdam take the lead; for the improvements of the hives, and ſtatement of their ſwarms, Thorley and White are to be conſulted; and for late diſcoveries in the propagation of bees, Schirach, Wilhelmi, Bonnet, and Debraw, are to be preferred. Whoever will be at the pains to refer to thoſe authors, particularly Reaumur's Hiſtory of Inſects, beſides the common place books, ſuch as the Academy of Sciences, Philoſophical Tranſactions, and Univerſal Dictionaries, will find,—that the whole account of bees, as given by John Hunter, is collated and made up, from the information of others,—from what has been already regiſtered. If this could not have been proved, reaſon would aſſure us of the fact; as whoever is deſirous of obtaining the truth, relative to bees, and of improving upon the obſervations of others, muſt employ, as moſt of thoſe authors I have alluded to, have declared they did,—a patient attention to the ſubject for a ſeries of years. Whether ſuch an exerciſe of his time, could have been actually proſecuted by John Hunter, I leave thoſe to ſay, who knew how all his hours were devoted. I do not doubt but he might have had hives at *Earl's Court*, and that he ſometimes ſlept there. This paper was publiſhed in the year 1792, and

I am certain, that moſt of its contents were made up very lately, as he could not have known—what Riem and Schirach's books had ſaid upon the ſubject, before they were tranſlated;—and I know *the German*, who tranſlated the papers for him, and the time when he did it. Beſides Debraw, whom he quotes, obtained the reading of his paper from the Royal Society, in November 1776: from this I infer,—that if John Hunter ſtudied the ſubject at all, he did it at a time of his life, when he had the leaſt leiſure for it.

He himſelf has borne teſtimony to the minute anatomical deſcription on bees, by Swammerdam: I therefore ſhall confine my obſervations to two points. Firſt,—to what he preſumes to be his diſcovery of the formation of wax; and ſecond,—to the diſcoveries made by Schirach and Debraw, which he has ſeverely reproached;—diſcoveries of the utmoſt importance,—and which, for the better aſcertaining the œconomy of bees, and the colonizing of them by art, muſt ultimately prove of the higheſt value.

I. Of the wax, he has ſaid,—that it is a ſecretion of the bees,—that it is their wax,—and that his is—*a totally new account of the wax*;—and that it is *formed by the bees themſelves*. It may be called

called an external ſecretion of oil; and I have found, ſays he,—that it is *formed between each ſcale of the under ſide of the belly*. But he adds,—that he has never caught them at taking the matter out of theſe ſcales. He makes an inference, which is oppoſed by the obſervation of other authors,—that they can, being provided with this wax by nature, work up their combs as well in wet weather, when they do not go abroad, as in dry. The beſt authors have ſaid otherwiſe: they have told us,—that the bees in about ten days, if the weather be fair, are firſt employed in forming the comb and compleating it;—and wax has been ſeen by Thorley eſpecially, within thoſe rings of the belly on bees, which have returned home laden.

To ſuppoſe—that a young ſwarm of bees, who have the whole of their comb firſt of all to provide, can ſecrete in ten days or a fortnight, when they are by bad weather confined to their hive, more wax in weight—than the whole ſwarm will weigh,—is, in my opinion,—to ſuppoſe an impoſſibility. And moreover, if the bees were thus confined at home by weather, how could a young ſwarm be furniſhed with *farina*, to work it up with their ſecretion? As after he has ſo roundly aſſerted,—that this ſecretion, is the wax,—he ſoftens down that by adding,—that *farina*

mixed with this ſecretion, forms the wax. To ſuppoſe that,—if his theory were true, wax would not be always found in the act of ſecretion, conſidering the quantity which muſt be ſecreted,—is alſo to ſuppoſe an impoſſibility. And to ſuppoſe,—that he, who has made this diſcovery, could not ſee the bees applying it, in the way he would wiſh to perſuade us—that it is applied,—is alſo to ſuppoſe another impoſſibility. But this is not all: I ſhall contravert John Hunter's being the diſcoverer of the idea,—prove that Riem has previouſly ſported it, and what is fortunate for me,—prove alſo that he had ſeen it. Riem's idea is quoted in Schirach's book on bees,—and neither John Hunter nor his tranſlator could have overlooked it, as it directly follows a Paragraph quoted by John Hunter. In page 240 of Schirach's French edition, and in a chapter under the title of

RESULTATS DES OBSERVATIONS 1764,

Article 3me. *Riem a obſervé l'accouplement —de la Reine, avec les Feaux-Bourdons,*

John Hunter has literally had this tranſlated for him,—as may be ſeen in my abſtract; and the ſubſequent article is that—which I have alluded to,—and ſhall here give the copy and tranſlation.—

Art. 4me.

Art. 4me. *Le Naturaliſte de Lauter affirme, qu'il a vû ſortir d'entre les anneaux des ouvrières de la matiére à cere : que cette matiére ſembloit tranſuder de l'interieur, et que c'eſt avec cette cere tranſpirée, qu' elles forment les commencements des cellules.*

The naturaliſt of Lauter affirms,—that he has ſeen flowing out from between the rings of the working bees, the matter for wax :—that the matter ſeemed to tranſude from within,—and it is with this wax thus tranſuded—that the bees form the commencement of their cells.

So far from this being a new diſcovery, it is mentioned alſo by Pliny ; and it having been ſo long expoſed to common obſervation, and by time gaining no credit, is a ſtrong reaſon for ſuppoſing it not to be true. It is very clear, that John Hunter was not the author of the diſcovery—and as clear—that he aſſumed to be the author of it.

II. I ſhall now revert to the ſecond point,—to the diſcoveries made by Schirach and Debraw, and which John Hunter has, without argument or proof, with ſo much aſperity, and indignity ſcouted. The outlines of the opinions adopted by former naturaliſts, were exactly like John Hunter's. They aſſerted—that the queen bee is the

the only female in the hive, and the mother of the next generation;—that the drones are the males by which ſhe is fecundated;—and that the working bees, or bees that collect wax in the flowers,—that kneed it, and form from it the combs and the cells which they afterwards fill with honey—are of neither ſex,—and which John Hunter calls mules.

But of late, Schirach has given a very different view of the claſſes which conſtitute the Republic of bees. He affirms, that all the common bees are females in diſguiſe;—that every one of thoſe bees, in the earlieſt period of its exiſtence, is capable of becoming a queen bee;—that the queen bee lays only two kinds of eggs, thoſe that are to produce drones, and thoſe from which the working bees are to proceed. Debraw has ſaid,—that the trials made by Schirach, ſeem to evince the truth, and adds—that he himſelf by trials alſo, is able to pronounce on their *reality*.

Debraw's ſubſequent diſcovery has moſt undoubtedly befriended Schirach's. As Debraw has ſhewn, how that which Schirach could not have accounted for, came to paſs. Debraw is to be commended for his candour, in allowing to Meraldi and Reaumur, the originality of the obſerva-

obſervation, which he ſo uſefully has applied; and although this had eſcaped Schirach, yet it could not annihilate the common occurrence obſerved by him, as nature will proceed uniformly in her operations, whether we can account for them or not—but we muſt firſt obſerve them, before we can think of accounting for them.

Debraw's diſcovery is founded, upon what has been ſaid before, by Meraldi and Reaumur:—the former ſaid, we have found a great quantity of drones much ſmaller than thoſe we had formerly obſerved, and which do not exceed in ſize the common bees, &c. &c.* And Reaumur has ſaid, we likewiſe have found drones that were no bigger than the common bees.† *Theſe* have been proved by Debraw to remain *during the winter* in the hive, and by *theſe* the eggs of the queen are *fecundated*,—inſtead of the queen being impregnated by the large drones,—as John Hunter has ſaid,—even after he was informed of this plain fact,—*before* the winter in the month of Auguſt,—and *ſix months* at leaſt before the ſubſequent ſpring,—which is the time that the queen is to lay her eggs thus impregnated.

* Royal Academy of Sciences, 1712, p. 333.

† Natural Hiſtory of Inſects, p. 591.

But

But what renders John Hunter's theory an abſurdity is,—that the old queen bee ſhould go thus impregnated *ſix months* during the winter, —whereas after the winter, neither the old queen, nor the young one, ſwarmed in the ſpring, go more than a *month*.

I muſt beg permiſſion to explain the trials of Debraw.

In order to aſcertain the fact, that the eggs are fecundated by the males,—he took a ſwarm of bees, and having ſeparated the drones, by ſhaking all the bees into a tub of water, and leaving them in it, till they were quite ſenſeleſs, replaced the working bees and their queen, as ſoon as they were recovered, by ſpreading them on a brown paper in the ſun, in a glaſs hive. The queen laid eggs, ſome of which, at the end of twenty days were hatched into bees, others withered away, and ſeveral of them were covered with honey. Suſpecting that ſome of the males, having eſcaped his notice, had impregnated only part of the eggs, he was anxious to aſcertain the fact; and with this view, he removed all the brood comb that was in the hive, and determined to watch the motions of the bees, after new eggs were depoſited in their cells.

On

On the ſecond day, he perceived the operation related in a former caſe by him, and to which I refer my readers;* and on taking out a piece of the comb containing two of the bees, which had thruſt the poſterior part of their bodies, into the cell, he examined them, and found, that they had no ſting; and upon diſſection, he diſcovered in them, by the help of a microſcope, the four cylindrical bodies, containing a whitiſh liquor, which Meraldi had obſerved in the large drones.

In a ſubſequent trial, Debraw ſeparated from the ſame parcel of bees, all that had no ſtings, and he found no leſs than fifty-ſeven of the number exactly the ſize of common bees, which on being preſſed between the fingers, yielded a ſmall quantity of whitiſh liquor. Having killed all theſe, the remainder of the ſwarm was reſtored to the hive. On the fourth or fifth day, the queen bee depoſited her eggs in the cells, but no part of the proceſs of impregnation could be diſcovered; the eggs, after the fourth day, inſtead of changing in the manner of caterpillars, remained in the ſame ſtate they were in, the firſt day, except that ſome of them were covered with honey: all the bees left their hive, and attempted to get into a neighbouring hive, proba-

* Vide Debraw's paper.

bly in ſearch of males; but the queen was found dead, having been killed in the engagement.

Debraw made another trial, which muſt be conſidered as abſolutely deciſive on this ſubject. He took a part of the unimpregnated brood comb, and placed it under a glaſs-bell, in which he confined a queen, and ſome common bees without any *drones*; the other part of the ſame brood comb, he put under another glaſs-bell with a a few drones, a queen, and a number of common bees. In the former glaſs, the eggs remained in the ſame ſtate; there was no impregnation; and when the bees were releaſed in the ſeventh day, they all flew away. The drones, in the other glaſs, were obſerved to impregnate the eggs in every cell on the day after they were put in; the bees remained in the hive, and in the courſe of twenty days, every egg underwent the neceſſary transformations, and a numerous young colony was thus produced.

It is with pleaſure I can ſay,—that theſe diſcoveries by Schirach and Debraw have made their way all over the *Continent*. John Hunter was never ſeen to worſe advantage, than through this paper. It conſiſts of forty pages in *Quarto*; and being upon a ſubject more expoſed to criticiſm than moſt of his others, his errors and crooked

crooked intentions are ſeen more glaringly by the world at large. He appears throughout the whole of the paper to be flat, wavering and equivocal;—conſtantly floundering like one who has found himſelf beyond his depth,—like a *fiſh* out of his Element—or rather a *man* not in his.—

THE FOLLOWING PAPERS ARE TO BE FOUND IN JOHN HUNTER'S BOOK "ON ANIMAL ŒCONOMY."

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1. OBSERVATIONS ON THE GLANDS SITUATED BETWEEN THE RECTUM AND BLADDER CALLED, VESICULÆ SEMINALES.

JOHN Hunter has ſaid, that the *veſiculæ ſeminales* have been conſidered as *reſervoirs* of the ſemen, ſecreted by the teſticles,—in the ſame manner as the gall-bladder is ſuppoſed to be a reſervoir of the bile;—but his analogy is not a juſt one. And although the *veſiculæ ſeminales* have been ſuppoſed to be for the reception of ſemen—previous to ejaculation by former authors,—yet thoſe who have ſaid ſo, have not juſtified themſelves, by having reſource, to ſuch an incomplete analogy.

For my own part I have my doubts upon the queſtion,—whether the *veſiculæ ſeminales* be for the purpoſe of a receptacle for the ſemen, or of ſecreting glands. But of this I am confident,—that the change he has made in their deſtination does not improve the truth,—does not clear up the fact—nor convince the underſtanding. For he has ſaid,—that the bags called *veſiculæ ſeminales* are

are not the ſeminal *reſervoirs*, but glands ſecreting a peculiar mucus, and that the *bulb* of the *urethra* is, properly ſpeaking, the receptacle in which the ſemen is accumulated previous to ejection. This is much more improbable than the former, and I cannot bring myſelf to give the ſmalleſt credit to it.

It may be preſumed—whenever a man has been worked up to the almoſt immediate and direct act of emiſſion, and has from any cauſe baulked the intention, ſo as not then to emit,—that the ſemen has been circulated through the *vaſa deferentia* from the *teſtes*, and found a receptacle *ſomewhere*; and as we know, that *ſometimes*, ſuch is the ſtate of the caſe, it is moſt probable —that the receptacle is in the *veſiculæ ſeminales*.

His plates annexed to this ſubject are very bad and unnatural.

2. ON THE STRUCTURE OF THE PLACENTA.

THIS offspring has been claimed by two fathers,—by both William and John Hunter. It has been publiſhed by William in the year 1764, and is to be found with the former diſputes annexed to it, in his *Commentaries*. But in conſequence of their having diſagreed, the right in it was diſputed, and conteſted *even* by the brothers,—who had joined in conteſting it with others,—about the year 1779. John ſent in his claim to the Royal Society; but as the fact had been given before, to the public, which the paper contains,—it was refuſed a place in the Philoſophical Tranſactions.

It conſiſts of an inveſtigation into the anatomical connection between the mother and *fœtus in utero*. As this ſubject has been ſo repeatedly bandied about, and ſo long demonſtrated by anatomiſts, in their ſucceſſive courſes of lectures, for a ſeries of years, not leſs than forty,—from 1754 to the preſent time,—I ſhall decline dwelling upon it.

The plate annexed to it, is a miſerable one:—It gives juſt as good an idea of the country in the *Moon*, as it does of that which it is intended to explain:—it will ſerve for either.

3. ON A SECRETION IN THE CROP OF BREEDING PIDGEONS FOR THE NOURISHMENT OF THEIR YOUNG.

JOHN Hunter has not told us, why this paper did not go into the Royal Society: I am ſure that the ſubject is more worthy, than that of the *new marine animal*, or the *curious pheaſant*. But he has told us in this paper,—that during incubation, the *coats* in the *crop* of the pidgeon are gradually enlarged and thickened, like the *udders* in female animals;—that the whole, except what lies on the *trachea*, becomes thicker, and takes on a glandular appearance, having its internal ſurface very irregular. It is likewiſe more vaſcular than in its former ſtate,—that it may convey a quantity of blood, ſufficient for the ſecretion of a curdy ſubſtance, which is to nouriſh the *brood* for ſome days, after they are hatched.

John Hunter was right to except the encreaſe of ſize in the crop about the *trachea*, for fear his theory ſhould *choke* the pidgeon; and he judged alſo wiſely in giving an encreaſed ſubſtance an encreaſe of blood veſſels, for the purpoſe of an encreaſed ſecretion. What nature in her ignorance does not do, his wiſdom ſupplies.

—She

—She can admire in him that which ſhe cannot accompliſh—but he does.

I do not believe this theory in the utmoſt extent to which he has carried it, and for the following reaſons. Firſt, that the cock pidgeon feeds the hen, and the hen feeds the cock: and ſecond, that ſometimes the cock not only incubates the eggs, but feeds the two young brood. Although John Hunter has gone very cloſe in this paper to ſay,—that the male and female both in their turns incubate the eggs and feed the young,—although he has not either poſitively denied or aſſerted theſe facts,—yet he has not ventured to aſſert—that the crop of the cock pidgeon undergoes the ſame anatomical change during incubation as he has ſaid—that the hen's does:—whereas for the confirmation of his theory, as cock and hen both alternately perform theſe ſame offices, *both* ſhould enjoy the ſame natural endowments. The two plates, which he has annexed to this paper, might be fairly deemed to be a deſign in the artiſt, for caricaturing his theory.

4. ON THE COLOUR OF THE PIGMENTUM OF THE EYE IN DIFFERENT ANIMALS AND THE USE OF THE OBLIQUE MUSCLES.

THIS ſubject is purely theoretical, and particularly adapted to the mind of its author,—furniſhing him with an opportunity, without comparative contradiction from others, of diſplaying the full energy of his genius,—and containing the beſt ſpecimen of his perſpicuity, with which he diſcuſſes different objects, and by that illuſtrates their truth.—As purely a piece of theory, without any practical purpoſe whatever, I will not attempt to ſearch after a cauſe for criticiſm: but I will politely and patiently wait until hereafter,—when the primeſt among all his admirers, ſhall be pleaſed to fix upon any points in this deſultory effuſion, where the truth of the theory, and the practical advantages reſulting from it, is to be found—challenging our admiration.

John Hunter has ſo rapidly, in this paper, ſhifted his arguments, if they may be ſo called, —has gone into the theory of viſion, ſo unintelligibly to common comprehenſion,—has ſo turned aſide the purpoſes of inveſtigation, by never leaving one ſingle point well explained—and

has ſo conſtantly brought forward ſucceſſions of ineffectual conjectures,—that the reader is left in one continued ſtate of diſappointment from the beginning to the end. His ideas are no ſooner hoped to be found out, than they vaniſh away in tangents,—retiring to their ſeveral and ſacred receſſes of the brain which gave them their creation. He marks out no direct path, but imitates a ſwallow in the air, when he is ſeen in the purſuit of inſects.

5. A DESCRIPTION OF THE NERVES WHICH SUPPLY THE ORGAN OF SMELLING.

BEING ignorant of the time when he publiſhed this paper—I cannot ſo well purſue his motive for doing it, as I could certainly have done, if I had known its date. The ſtrong preſumption is,—that he publiſhed it after *Scarpa*, the profeſſor of anatomy in *Pavia*, had publiſhed on the ſame ſubject, and which was in the year 1782:—and by John Hunter's recurring to his uſual reſource of *notes* made ſo far back as the year 1754, I am apt to think,—that my opinion is right,—and that this paper was not publiſhed until ſince the year 1782.

After having gone into his accuſtomary proofs of originality, and entertained himſelf in the moſt liberal manner, with the *notes* which were made, and the demonſtrations which were ſhewn, in the many courſes of ſucceſſive lectures given by the brother William, from the year 1754,—and after having amply gratified his natural deſire for proving an inſufficiency in the knowledge of Willis, Winſlow, and de Haller, upon this ſubject, he proceeds to his demonſtrations from the reſult of diſſections.

I can only ſay of this paper,—that he has not ſatisfactorily determined the queſtion relative to the diſtribution of the *firſt* pair of nerves, nor the purpoſe of that branch of the *fifth* pair which he has alluded to: if this aſſertion be doubted,—let any candid perſon refer to his paper, which muſt ſpeak in this caſe for itſelf.

There are two plates annexed to this paper, moſt elegantly executed: but I do not pledge my opinion,—that they convey a perfect repreſentation of the thing in nature, for which he has thus expenſively diſplayed them.

OBSER-

OBSERVATIONS ON THE INFLAMMATION OF THE INTERNAL COATS OF VEINS, READ IN FEB. 1784, AND PUBLISHED IN A VOLUME ENTITLED TRANSACTIONS FOR THE IMPROVEMENT OF MEDICAL AND CHIRURGICAL KNOWLEDGE, 1793.

THE purpoſe of this paper is to announce, that a vein in the arm from the operation of bleeding, ſometimes becomes inflamed; and that tis poſſible, the inflammation may proceed along the internal coat of the vein through its whole direction, extend to the heart, and thereby kill the patient. The indicative cure recommended for this alarming caſe, is ſuggeſted in conſequence of its having been *once* practiſed by John Hunter, and, as he ſuppoſes, with ſucceſs. "When inflammation takes place beyond the orifice, ſo as to alarm the ſurgeon, he ſhould immediately make a compreſs upon the vein at the *inflamed* part to make the two ſides *adhere* together, &c."

He ſays nothing about the common means for reducing inflammation.

This paper is of no other importance, than that it relatively is capable of being, from example, fraught with miſchief. If it had, in a general way, deſcribed the nature of inflammations,

tions, which ſometimes follow in conſequence of opening a vein in the arm by a lancet, and pointed out—how far the treatment already eſtabliſhed upon the ſoundeſt principles of ſurgery, for the cure of theſe common caſes, was injurious to this uncommon caſe;—if it had nicely and critically marked out a diſtinction, between the mode of treatment univerſally adopted in common caſes,—and if it had drawn a neceſſary line by which inflammation on one anatomical part, differed from that on another, and for which, a different practice was neceſſary in the treatment of each,—I ſhould have then been enabled with more facility to diſtinguiſh, what at preſent I find myſelf under ſome difficulty in accompliſhing to my ſatisfaction, namely—that eſſential difference which this piece of novelty aims at.

I can, in peruſing this paper, find no inſtructions authorizing the ſurgeon who reads it, with any intent to obey it, how to diſcriminate betwixt an inflammation in the vein, and an inflammation on the other parts of the arm. It muſt neceſſarily follow therefore,—when this paper is peruſed by a practitioner who is not already eſtabliſhed in his own opinion, but is guided in his practice—from what is told to him by others,—that if he believes what John Hunter

ter in this paper would feign to inculcate,—he will be implicitly and indiſcriminately applying a tight bandage to compreſs the inflamed vein, whenever an inflammation follow the operation of bleeding by a lancet.

If this paper had announced—that the inflammation in the vein from bleeding, required a different treatment from other inflammations, which ariſe from bleeding,—and had alſo explained to us—that the treatment to be adopted in this caſe, was not intended to interfere with the more general mode of treatment, as adopted in all other inſtances of inflammation,—ſuch as relaxing the arm, bleeding, fomenting, poulticing, and exhibiting medicines calculated for the abatement of fever, conſequent to local inflammation,—I ſhould then have been enabled to have met the queſtion in its pureſt abſtracted ſenſe,—whereas I have now to combat it, in one of a more compound and intricate nature.

I have to appeal to the moſt eſtabliſhed and ableſt ſurgeons,—whether John Hunter has not, by thus directing a compreſs to be laid on the arm, for the avowed purpoſe of ſtopping an inflammation in the vein, held out to practitioners in general, a ſyſtem tending to produce the moſt alarming conſequences;—or whether he has,

has, throughout this paper, explained any difference, ſo that it can be decided with ſafety and certainty, when this compreſs on the vein is proper, and when it is not:—Innovation ſhould be clear, and when it is not, it is dangerous in ſurgery.

The worſt caſes of inflamed arms are found to ariſe from a long neglect of the inflammation; and ſuch have been adduced in his late publication by Abernethy of St. Bartholomew's,—a young ſurgeon whoſe genius, though yet in its bloſſom, promiſes hereafter the choiceſt fruits of ſcience cultivated on a mind, richly endowed by nature. The treatment of thoſe caſes, ſome of which were bad—as adopted by Pott, and—as recited by Abernethy,* was exactly that ſort of treatment, which will be ever ample, for the cure of every aggravated ſtate of inflammation of the arm, independant of that brought on from virus, whenever aſſiſtance be had in time: and when ſuch a mode of treatment does not ſucceed, it is not becauſe it is not the beſt,—but becauſe the caſe had been too long neglected: the danger, there, ariſes from procraſtination, and not from want of remedy already eſtabliſhed.

* Vide Surgical and Phyſiological Eſſays.

Having

Having thus far explained myſelf,—I ſhall revert to this anatomical and newly diſcovered diſeaſe, examine into its poſſible and probable conſequences, and enquire—whether it poſſeſſes in its true nature, ſuch fatal diſtinguiſhing marks, —as to warrant an innovation in practice—and as to demand a mode of treatment, which, in my opinion, would be productive of a general miſchief, more fatal, than if the diſtinction had never been made, or, the innovation ſuggeſted.

In the caſes of inflammation from bleeding, deſcribed by Abernethy, or rather ſketched out by him, for he has not gone into the particulars of their treatment, they *all did well* without a compreſs; and it does not appear that he did any thing for them, out of the common way in ſurgery. But notwithſtanding he has found,—that he could have diſpenſed with this novelty, yet he has ſaid,—that "the application of compreſs, at ſome diſtance from the punctured part, in order to unite the inflamed ſides, appears to be perfectly judicious." He has alſo ſaid,—that "the inflammation of the venal tube is extenſive; and it is indeed very probable, that much ſympathetic fever will enſue; not merely from the excitement which inflammation uſually produces, but alſo, becauſe irritation will be continued along the membranous lining of the vein

to the heart." And to do him juſtice, he has ſaid—what John Hunter has not—" that the nature of the diſeaſe being known, the treatment is commonly evident. The diminution of inflammation in a vein, is to be attempted by the ſame general means, as in other parts." I am afraid,—that he has deſcribed ſuch an inflammation, as will not admit with ſafety any compreſſion, or as cannot be borne with any poſſibility of good effect. It is apparent,—that the motive in John Hunter and in Abernethy is, to compreſs the inflamed vein—that the inflammation of the internal ſurface of it, may not extend to the heart, and by that kill the patient;—but can it with propriety or ſafety be done?

When the vein is thus inflamed, it appears from the caſes recited,—that the vein is not ſingly inflamed, but other parts in vicinity to it are ſo alſo;—and that the whole of the ſymptoms are worſe than from any other part, being the original ſeat of inflammation: this is atteſted by my quotation from Abernethy, and alſo, —that the irritation produces fever and tumour of the arm, in an exacerbated degree, to inflammations not peculiar to veins. I think, it was very fortunate for thoſe caſes deſcribed by Abernethy,—that they got well, *before* the idea of a compreſs had been imparted —and

—and that they only left behind them, the ſlight imperfection of a vein collapſed, by what he terms, adheſive inflammation, which is a *Barbariſm*; for it is, adheſion in conſequence of a degree of inflammation.

Hence it appears,—that the inflammation is in the ſame degree, if not in a worſe, on arms, where veins are ſaid to be affected in particular, than on arms, where veins only ſhare the common inflammation. For as the cauſe originates in vein, it cannot at any time and in any caſe be ſaid to be exempted. And it appears alſo,—from the whole which I can collect from the deſcriptions of caſes by both theſe authors—that they are more calculated to diſplay anatomical diſtinctions, and the variety of inflammations ariſing out of them, than to eſtabliſh, upon a more improved foundation, the practice of ſurgery,—although the latter ſeems to be the avowed intention of both for writing their Eſſays, or if it be not, there can be no other motive ſo good. Therefore I ſay, from what I can collect out of theſe deſcriptions, without aſſenting to or denying,—that an inflammation within the vein as is deſcribed by them, does really or not exiſt,—I am confident in the opinion,—that if ſuch a caſe be attended to in time, it will always do as well, as a caſe of a more common de-

ſcription,—and that, *ceteris paribus*, there is no more danger to be dreaded from *this*, than from any other.

I ſhall now examine, when this compreſs is to be put on the vein; and as John Hunter has not directly told us, when, the ſeaſon muſt neceſſarily be collected from inference. It ſurely cannot be put on, before inflammation has appeared, as then, every one who is bled ſhould have one;—and to put it on, after inflammation has ſubſided, would be an act of ſupererogation. Of courſe the time then for putting it on is—when there is danger of the inflammation ſpreading,—when the arm is ſwollen,—when the fever is raging,—and during the acts of relaxing the limb by fomentation, poultice, bleeding, medicine, and indulging to its utmoſt extent, freedom to the parts, and eaſe. And further, the compreſs muſt be put, on a part in the actual ſtate of inflammation,—or otherwiſe, adheſive inflammation as it is called, cannot take place.

I think this is *a ſort* of a dangerous kind of practice,—that it is an innovation unwarranted by reaſon, or the true principles of ſurgery;—and that ſuch a treatment is more calculated to produce fatal effects, than any proſpect of inflammation

flammation reaching the heart of a patient, when he is under the care of a plain ſurgeon.

Nor am I convinced—that there is a poſſibility of the matter formed within the vein being driven by the laws of circulation into the heart :—if it be true, that the vein does thus in ſome caſes ſuppurate internally, for ſome length, ſurely a vein thus conditioned, muſt be rendered incapable of carrying on the circulation either of blood or matter.—Its valves are likely to be deſtroyed, and its capacity to be cloſed up.

Of the adheſion of veins, after an inflammation by bleeding, there has been a profuſion of inſtances produced : but yet I think theſe inſtances do not ſo frequently offer to common obſervation, as from ſo much having been pledged about adheſion, there would be reaſon to infer. I have particularly noted, whenever there has been, after bleeding, a ſlight abceſs of the orifice, ſo as to cauſe a loſs of the cellular ſubſtance under the ſkin, and to leave an indentation in the arm, ſo as to admit a ſubſtance as large as a pea to lodge within it, and fill up the hollow,—that although the vein did not riſe, when tied up for another bleeding, at another time,—yet the vein is always to be felt, and has not

not at leaſt hitherto, ever yet been found *by me*, —not to yield blood after the lancet.

Upon the whole, there ſeems to have been a diſplay of anatomical and theoretical conjectures gone forth, and which has not only attracted attention but alſo excited curioſity, from the rare inſtance of its author having written on any part of ſurgery, or on any ſubject applicable to ſurgery ſo immediate as the preſent. To thoſe who never will apply the compreſs, the diſtinction will remain without a difference in practice; and to thoſe who will apply it, who will at all events worſhip the Golden Calf of Leiceſter-ſquare, I have only to ſay,—that, in this inſtance, I ſhould fear their temerity might be compenſated by the loſs of reputation. They might, it is true, by a compreſs thus conditionally applied to a vein, ſtop the progreſs of inflammation to the heart, and bring on thereby mortification in the arm.

Here my explanatory remarks are brought to a concluſion. What Baudius ſays of Eraſmus ſeems applicable to John Hunter :—Magis habuit quod fugeret, quam quod ſequeretur.

PART

PART IV.

SERIES OF TRANSACTIONS FROM 1770 TO THE FINAL CLOSE;—WITH AN ACCOUNT OF THE PROGRESS AND ARRANGEMENT OF HIS MUSEUM.

WHERE a life has been totally engaged, and every hour of it, even, has been abſorbed, in the variations of natural hiſtory, anatomy, and ſurgery, if theſe points had been excepted, there would have remained nothing to have been written upon. When John Hunter was abroad, he was conſtantly thoughtful about that he had left to be done, on his return home; and perhaps, there cannot be found his equal, who ſo completely filled up time, in active induſtry. As his works are in ſuch high eſtimation with his admirers,—as theſe conſtitute him, in their opinion, the ableſt phyſiologiſt and ſurgeon in the world,—as his admirers are perſuaded that his works will be read, as long as the arts of the preſs, the type, paper, and ink, hold together, —ſurely then a comment upon them, muſt have been exacted from ſomebody.

In 1770, John Hunter had the honour conferred upon him, of Surgeon Extraordinary to his

his Majeſty. In the following year, he was married to Miſs Home, the daughter of a ſurgeon: ſhe has borne him a ſon and daughter; the former is entered in the Temple, and the latter, at preſent, is at home with her mother. To her he was directed, not only by perſonal attractions, but alſo mental endowments, which ſhe poſſeſſes in a very eminent degree. She has exhibited ſpecimens of poetry in ſonnets, which for beautiful fancy, and pleaſing harmony, are excellent in their ſtyle: and from the blandiſhments of her natural diſpoſition, he found the cares and aſperities of his life ſoothed to the end,—as long as his heart continued to vibrate. She was to John Hunter, what his Mariamne was to de Haller; but the abrupt ſtroke of death deprived de Haller too ſoon of that boſom comfort, by which life is endeared, which he mourned in accents of the moſt plaintive and melodious poetry, and which, for a long time, ſaddened all his purſuits.

To unbend the mind from that Tedium which, during the ſummer months, comes over every man of care, ſtationary in this Metropolis,—to refreſh the animal functions, half poiſoned and debaſed, by anatomical miaſma,—and to be as little as poſſible out of the way of the ſudden calls of a ſurgeon, John Hunter choſe a cottage at

at *Earl's Court*, about a mile in the midſt of fields, beyond *Brompton*. There he ſometimes retreated for freſh air, and took his hobby horſe along with him. Nobody of common curioſity could have ever paſſed this original cottage, without being obliged to enquire, to whom it belonged. By obſerving the back of the houſe, a lawn was found ſtocked with fowls and animals, of the ſtrangeſt ſelection in nature,—as if it had been, another repoſitory belonging to Brooks;—and in the front, there were to be ſeen four figures in lead or ſtone, repreſenting Lions,—two in a form *paſſant* placed upon the parapet;—and on the ground, two more *couchant*, guarding the double flight of ſteps, leading to the veſtibule. On the ſides of the area, were ſeen, two pyramidal collections of ſhells, of a very contracted baſe, and mean height,—each of them, ſeeming to conceal a ſubterraneous entrance to a Golgotha. Over the front door was preſented the mouth of a Crocodile, gaping tremendouſly wide,—

—To gorge with blood his barbarous appetite.*

And to prove, that there lived a Philoſopher within this humble retreat, and that a flaſh of Lightning will equally dart, on the roof of a cot-

* Dryden.

tage of a ſurgeon, as on the turrets of a Palace of a Prince—there were placed erect, high above each gable wall, electrical conductors, daring its temerity. Here it was that John Hunter dreamed over many of his projects,—realized experiments on animals,—and laid the foundation of his *Fable of the Bees*;—and here was his country reſidence during his life time.

Here it was, that he paſtured thoſe Buffaloes which he ſo lately, as in 1792, put into harneſs, and trotted through the ſtreets of London,—not judging, that he might have been fairly outrivalled, by a ſhowman's Dromedary,—eſpecially, if there were, and probably there would be, the additional effect of a Monkey mounted on his back,—playing its little antic tricks. Savage beaſts, ſaid to have been ſnared, on the lofty and arborous mountains of Thibet, or on the dreary wilds of Boutan, and imported here, for autumnal exhibition, on carnival days at Smithfield, held in honour of St. Bartholomew, were ſure to be firſt ſhewn to John Hunter;—their cunning paraſitical keeper—prodigal of his illuſtrious name,—thus enhancing the eſtimation of his rare Aſiatic curioſities.—We are alſo told, that Giants and Dwarfs, were certainly retained by him for diſſection,—whenever the fates ſhould ſo determine it—whenever the ſiſters ſhears

ſhears of deſtiny, ſhould cut the threads, on which their lives ſuſpended.

From 1770 to 1780, John Hunter's profeſſional profits did not keep pace with his expences; and theſe ten years were particularly preparatory for obtaining information, and acquiring fame, that were hereafter to raiſe him to eminence, and reward him for all the toilſome labour, which every hour was engaged in. This might be deemed his probationary æra. His printed productions were during this period, proſecuted with uncommon ſedulity, and his Muſeum was conſtantly gaining acceſſions. Beſides theſe engagements, he at laſt found out—that it was neceſſary for him, in order to be great, to become a Lecturer; and that which he ſhrunk from, when he abſconded the lecture-room, and entered into the army, he found himſelf now bold enough to undertake. In the autumn of 1773, he advertiſed,—not a courſe of anatomical lectures, but a ſort of a ſkirmiſhing courſe—ſomething new, and which could not be compared,—conſiſting of ſurgical, phyſiological, and comparative anatomical branches,—and ſo mixing them together, as either to confound or illuſtrate each other. There were two unuſual circumſtances attending this annunciation:—his terms were high, and his introductory lecture was not open.

I recollect having called for his ſyllabus, and thus finding, that the deſign was not liberal, from that cauſe only, declined being his pupil; what I gained, or what I loſt, can be only decided by thoſe opinions, which he has publiſhed, and of which I have diſapproved.

Theſe lectures were continued at his houſe in Jermyn-ſtreet, with very unequal ſucceſs; and his diſſecting room was opened alſo, under the ſame capricious viſitations. To ſome of his courſes, I have been told, he had nearly fifty Attendants, and I have been alſo told,—that in the autumn of 1786, after the publication of his work on the venereal diſeaſe, he had, but twelve. To imagine even—that this undertaking was carried on with equal facility by John Hunter, that it could have been, had he been properly educated,—would be romantic; more eſpecially as nature had been very ſparing to him, in the gift of elocution. It was from this cauſe, or a much worſe,—that he lectured at home, and not at the hoſpital: inſtead of lecturing at the hoſpital, free of expence to its pupils, as was done by Pott, and of openly imparting his ſyſtem to thoſe, who were deſirous of encreaſing the ſtock of ſurgical knowledge,—he could, by thus lecturing at home, ſhut out every one capable of comparing his dogmas with eſta-

eſtabliſhed doctrines,—infuſe without contradiction his principles into the minds of his pupils,—and take their money into the bargain. Purchaſed ſcience is of more value than that obtained, as a gratuitous boon, from a ſurgeon of an hoſpital by his pupils:—thus thought John Hunter, but not ſo—Percival Pott.

In the beginning, theſe lectures were written on detached pieces of paper:—and ſuch was the natural confuſion of his mind,—that he would be frequently found incapable of explaining his own opinions, from his notes:—and after having in vain tried to recall the tranſitory ideas, now no longer floating in the mind, nor obedient to the will,—after having in vain rubbed up his face, and ſhut his eyes, to invite diſobedient recollection,—he would throw the ſubject by, and take up another. Although the greateſt part of the contents of his lectures were afterwards copied fair, by another hand, yet—upon every new opinion, in every freſh courſe, which his imagination had ſuggeſted,—ſcraps of papers were thus conſtantly produced, and conſequent embarraſſments, as conſtantly experienced in the explanation of them. It was in theſe predicaments—that he deſired his pupils not to take notes, or if they had, to burn them, for that probably

bly, in the next ſeaſon, he ſhould find cauſe for changing his opinions.

The purſuit of his lectures, and the illuſtrations he brought to his aid, from the lore of comparative anatomy, made him, with more eagerneſs encreaſe his ſtock of preparations;—a ſtock adapted to anſwer two purpoſes,—to demonſtrate out of it, to his pupils,—and to ſhow it to thoſe who admired moſt, what they leaſt underſtood. His firſt floor, and back apartments, were filling apace,—inſomuch that he was not able to find room, for the *Camela Perda* given him by Lady S——, the talleſt animal known, and which browſes upon the branches of trees: he therefore,—that it might be in ſight,—cut off its legs, and fixed it, in the paſſage.

By John Hunter being well grounded in anatomical practice,—by his conſtantly producing fruits of inveſtigations, in ſucceſſions of papers ſent into the Royal Society,—by the fair reputation he had of an anatomiſt,—by the foul application he made of that, in taking every poſſible method to inſinuate to the world, in the language of an egotiſt, that there was not his equal,—by his never ſtirring about any branch of the ſciences, without apprizing the public of its importance,—he thus depreſſed the modeſt merit

merit of others, and exalted his own. It is for this reaſon, I am aſhamed, for the honour of human nature, to own and repeat,—that, had his education been more liberal, or his heart more poliſhed,—he never would have gone the length which he did, nor ſucceeded ſo highly. If a body were to be embalmed, John Hunter was ſent for,—if a virtuoſo ſolicited a diſſection or preparation, to him he applied,—if any thing ſtrange in nature occurred, the explanation of it came from him. In theſe articles, whether the object aroſe from curioſity, or admiration for knowledge—he found himſelf equally ſought after: and as his vanity was flattered,—he willingly diſſipated this waſte of time;—thoſe then who thus employed him, not recurring to him for the opinions of a ſurgeon,—as that was the laſt and lateſt reputation he could eſtabliſh, or in the obtainment of which, he ſucceeded.

During the practice of Hawkins, Bromfield, Sharpe, and Pott,—the ſurgical engagements of John Hunter, were limited within the recommendations of thoſe pupils, who had known him in his brother's diſſecting room, and who were now ſcattered to various places, at home and abroad. The chief operations performed by him, at this period, out of the hoſpital, conſiſted in ſuch undertakings, as the judgment of able ſurgeons had

had induced them to decline. Such deſperate caſes, John Hunter was never found to heſitate in embracing: he ſought for the opportunity rather than rejected it, whenever it offered. A half pay Officer, reſident in Weſtminſter, had a ſpecies of Rupture, for which he had conſulted both Pott and Watſon, and both declined performing the operation,—knowing how ſtrong the chances were, againſt the ſucceſs. The patient was too eager for relief, to permit himſelf to reſt ſatisfied, with the judgment of theſe ſurgeons;—John Hunter performed the operation, and he died the next day. This was his reaſoning,—if I do not ſucceed, I cannot be blamed, as opinion was,—that the caſe would not admit of ſucceſs;—but if I ſhould ſucceed, I ſnatch a leaf of laurel, beyond the reach of thoſe who deſpaired of the poſſibility.—If I fail, it may not be generally known, but if otherwiſe,—it ſhall be.

At this period I know—that his conſtant habit was, to receive his patients in the morning, —without any decent preparations for their viſits. Sometimes he was found, with his hands beſmeared in the act of diſſection, and ſometimes,—after having waſhed his hands,—in putting on thoſe coverings of linen, over his wriſts, —which are commonly called keep-cleans, or cover-

cover-fluts. All thoſe ſcenes had their effect: the ſuppoſition that a man, thus engaged, was moſt capable of giving the beſt opinion, in a caſe of ſurgery, was generally believed. I have frequently ſmiled, on hearing the opinions, that men of liberal educations even ſported, upon the engagements which they found John Hunter employed in. They could never have been brought to conceive, from their own judgment, but that he muſt have been, the firſt, in his profeſſion;—and it would have been fooliſh to have attempted to correct that judgment,—by aſſerting, that decency and knowledge were not incompatible;—and that the ſurgeon, who had cultivated the ſcience of anatomy, in its proper time and place,—who had the underſtanding, to throw the foulſt appearances of the anatomical proceſſes, into the back ground,—and to impart by practice, the reſult of them, only,—was a wiſer man, and conſequently capable of giving a better opinion: As one occurrence will bring to the recollection, another in ſimilarity,—I have thus frequently aſſimilated this trait in John Hunter, to that ſuggeſted by Richard the Third,—when he contrives to be ſeen by the Lord Mayor, and Court of Aldermen, with a prayer book in his hand, and in an apparent ſervor of devotion.—The ſtudy of Pott had all theappearance of, one, belonging to a private gentleman; and his per-

ſon was neatly fitting, for the reception of his patients. He needed not the borrowed aid, of viſible ſigns of anatomy, for proving,—that he had ſtored his mind with all that was neceſſary, for perfecting his profeſſional knowledge. But ſuch are the tricks which lead the minds of the ignorant into captivity.

I am now to tell of a very unpleaſant miſunderſtanding, which took place, between the two brothers—William Hunter and John,—ſometime before the cloſe of the year 1780. In the minds of many—the cauſe will be deemed of too trifling a conſequence, to ruffle the ſpirits for a moment:—but trifles, in little minds, are always viewed through a magnifying medium. If I underſtand the cauſe of quarrel aright,—it aroſe, from John Hunter having invited William, to the ſight of a diſeaſed part of a ſoldier, who had died in conſequence of it;—and William having found,—that this diſeaſed anatomical property, would prove a valuable preparation for his muſeum,—cauſed it to be taken to his houſe, and refuſed to give it up, to the claim made by John. This was reſented by John, and this proved to be ſo ſerious a foundation, for the ſeparation of friendſhip and affection between two brothers, as never afterwards found any abatement. John enlarged the wound, thus made,

by

by demanding from William, his claim to an anatomical right, on the ſtructure of the placenta, which William had publiſhed in his Commentaries in 1764, and John afterwards in his Animal œconomy. To this claim made by John, William replied,—and to that, John gave in a rejoinder. The Royal Society received their papers, but proceeded no farther into the merits of the queſtion betwixt them. William Hunter dying, about three years afterwards, left his property away from John, and placed the ſuperintendance over his Muſeum, into other hands.

An aggreſſor in a quarrel, is rarely the firſt prone to forgiveneſs. Whether John Hunter diſcovered much worldly prudence, in his conduct on this affair, I ſhall not ſay; he was not rich, and his brother was: but this I will aſſert, that in this quarrel, the whole of the manly deportment, was on the part of John. It is not, becauſe one man is dependant upon another, to be reconciled by reaſon, or morality,—that an inferiority of fortune, ſhould force him to compound with an inſult founded in an act of injuſtice. Nor is it the value of the right, which conſtitutes the degree of injury, done by one to another, but the overbearing audacity of the act. William, palpably, aimed to take advan-

tage of John's dependance; and John's ſtate of mind was not accommodating enough, to ſubmit to it. But, notwithſtanding all this, John was found aſſiſting his brother in chirurgical offices, during his laſt illneſs; and perhaps he was then ſeen in the diſcharge of a practical duty,—in a ſituation to be envied—by the beſt man, that ever was born!

William had a querulous and hypocritical way of uttering his complaints; and he would deſcribe the imaginary injuries, done to him by others, with all the inſinuations of the worſt of crimes. He had the art of making out a plauſible ſtory, againſt his enemies, ſuggeſted by his native jealouſy. He ſtrongly reſembled in this part of his character, the little Bard of Twickenham. John could never talk of his injuries; but would get rid of the paſſions they provoked in him,—by the bittereſt utterings of ſwearing—Sterne's Dr. Slop could not have been a match for him.

In the ſpring of the year 1781, the theory of John Hunter's paper—on the Digeſtion of the Stomach after Death—and the advantageous knowledge reſulting, from the thouſands of experiments, which he boaſted of having made,—were brought into practical realization. He was

was retained, by a gentleman of the name of Nolan,—the friend of the late Captain Donellan, who ſuffered execution for the murder of Sir Theodoſius Boughton—to attend as a witneſs, at his trial, at Warwick aſſizes,—and he accordingly went. He was expected to be conſidered, as a ſecond Daniel—to ſave this arch-criminal, —and to bring him purified,—out of the fiery furnace of juſtice. I am afraid to expreſs my ſentiments, upon this act of John Hunter, by going into particulars. I very well recollect, how his conduct was conſidered, in the opinion of the day,—and I know how I viewed it then—and what I ſaid upon it. But as I have made up my mind, not of myſelf, to diſcuſs this tranſaction, by recurring to any comments I might have then formed,—I ſhall, in their ſtead, produce the ſummary of his evidence by the judge, —from the trial, that was publiſhed.

" For the priſoner you have had one gentleman called, an able man, and who is likewiſe of the faculty.

" I can hardly ſay what his opinion is, for he does not ſeem to have formed any opinion at all of the matter. He at firſt ſaid he could not form an opinion, whether the death was or was not occaſioned by the poiſon, becauſe he could conceive

conceive that it might be aſcribed to other cauſes. I wiſhed very much to have got a direct anſwer from Mr. Hunter, if I could, what, upon the whole, was now the reſult of his attention and application to the ſubject; and what was his preſent opinions; but he ſays, he can ſay nothing deciſive. So that upon this point, if you are to determine upon the evidence of the gentlemen who are ſkilled in the faculty only, you have the very poſitive opinion of four or five gentlemen of the faculty, that the deceaſed did die of the poiſon. On the other ſide, you have what I really cannot myſelf call more than the doubt of another; for it is agreed, by Mr. Hunter, that the laurel water would produce the ſymptoms which are deſcribed."

After mentioning the names of the faculty, whoſe opinions were poſitive—it will be needleſs for me to ſpeak of their reputations: Aſh, of Birmingham, Parſons, of Oxford, Rattray, and Wilmer, of Coventry:—three of whom, had made experiments on laurel water, and were thereby enabled to ſpeak, from its effect:—but I will have done of the ſubject!

The great acceſſion of articles, pouring into the Muſeum,—the death of William Hunter, excluding all collateral proſpects—the reſigna-

tion

tion of practice by Hawkins,—and the threatened decline of it by Sharpe—opened to John Hunter, plans, and proſpects, more extenſive than any he had hitherto formed, or conceived. He was reſolved not to be outdone by his brother, in the eſtimation of his Muſeum; and not to be depreſſed, but exhilarated rather, from the unnatural delinquency he had lately experienced. He was determined to keep up the appearances of profeſſional eminence, by anticipation; and to be a candidate for profeſſional preference, whenever vacancies gave him, the pretenſion. He therefore found—that the houſe in Jermyn-ſtreet contained not dimenſions, ſufficiently capacious, for the plans which his active mind ſuggeſted; and in 1783 he took a houſe, upon a much larger ſcale, in Leiceſter-ſquare, about the middle of the eaſtern ſide, which extended through, into Caſtle-ſtreet. This was fitted up in a very expenſive manner;—and here he eſtabliſhed an expanſive room for his Muſeum,—another, for a public medical levee, on every Sunday evening, —another, for a lyceum for medical diſputation, —another, for his courſe of lectures,—another, for diſſection,—another, for a printing warehouſe and a preſs,—and another, for vending his medical works.

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This undertaking could have been, alone, attempted but by a man of enterprize; and it could, alone, have had a proſpect of being proſperous, but by his natively poſſeſſing more intrepidity, more induſtry, and conſequently more credit,—than perhaps any profeſſional man of his own, or any former time. I do not wiſh to go into John Hunter's private affairs, beyond what is neceſſary to juſtify my declarations on his public, and therefore I ſhall ſhortly ſay,—it was not, becauſe his profits from practice authorized him to engage in this undertaking, that he embarked in it, for as yet his practice was far from being the greateſt,—but it was, becauſe this was the moſt probable chance, which offered amongſt others leſs deſperate, and conſequently leſs likely to ſucceed;—as deſperation was the ſtake of John Hunter, at that period of time.

His whole reliance was, upon the opinions of men; and theſe were to be obtained, by the appearance of things. The popularity of a ſurgeon, had long engaged his attention. To have retired from among the foremoſt, when vacancies were offering,—would have been in ſome meaſure,—to have declined his own purſuits,—to have renounced his firſt object,—and to have been out of the ſight of that public, after he had thrown himſelf ſo much in their way. It would have

have appeared, as though he were not to be found, when he was called upon, by the clamorous voice of popularity, and when he had given in his preferable claims;—claims which have promiſed ſo much, and have proved ſo little.

The new ſituation he had choſen, was convenient and centrical: and from this time, fortune ſeemed highly diſpoſed to favour all his projects, and implicitly to ſurrender her froward controul, over any of his adventurous engagements. Every thing that John Hunter now did, was conſidered by the public in general, as being the beſt poſſible method, in which every thing could be done. This is very ſtrange, yet it is very true, that miſcarriages—which fairly ought to have been attributed to an inferiority in knowledge, and not to an inevitable conſequence in the nature of the thing,—where the beſt means which were known, had failed from an impoſſibility or inadequacy in their power,—never affected the reputation of John Hunter. He could do ſuch deeds, without impeachment of character, as would have deſtroyed the reputation of any other ſurgeon. Whatever has been done by him, and which has, by its notoriety, allowed of an open inveſtigation from thoſe, moſt competent to compare it, with what could have been done by others,—has uniformly and

conſtantly convinced me,—that John Hunter poſſeſſed not the common talents, for common practical ſurgery. This aſſertion can only be proved, by the reſult of his practice; and I do not heſitate to ſtake my reputation, by ſaying,—that from the numerous inſtances of raſhneſs or inſufficiency which I could adduce,—I am authorized to pronounce him, to have been,—a very inferior, dangerous, and irregular practical ſurgeon.

In the month of December 1784, two caſes offered to John Hunter, from the bites of the ſame mad dog: one—of a young gentleman, Maſter Rowley; and another—of a French woman. Both of them were bitten, by the ſame dog, and at the ſame time. The accident happening in Jermyn-ſtreet, the young gentleman was immediately ſent to John Hunter. Reputation had directed where he was to go. The conſequence of this bite was,—that he died in January 1785, of hydrophobia. The woman, whom John Hunter likewiſe treated, died alſo. I do aſſert—that John Hunter did not do, for theſe patients, the beſt that could have been done, by the art of ſurgery—and that the miſcarriage was not owing to the inevitable nature of the caſes. The bite was inflicted upon Maſter Rowley's lip; and in the application of cauſtic

cauſtic to it, he offended againſt the principles in ſurgery, two ways.—Firſt, againſt the eſtabliſhed principles, by uſing the cauſtic, inſtead of the knife; and ſecond, becauſe cauſtic was not, when applied to a wound of that deſcription, adequate to the certain extinction of the poiſon.

If another had done thus,—or if another had done many other things, which he did—his reputation would have not only fallen into contempt, but a ſevere reprobation would have gone along with it. But this was not all,—he had the unfeeling effrontery, to enter into a public correſpondence, with a phyſician in Suffolk, about the ſymptoms of Maſter Rowley's death; as if there had been ſucceſs in the event of the caſe. This phyſician had not attended Maſter Rowley, but had obtained his information, without aſſigning his motive, from Tuſon, a ſurgeon of good reputation, and who attended the family. Tuſon was never given to underſtand—that the information was to go forth to the public—and therefore, when this indiſcreet correſpondence, painful to the feelings of the father, Admiral Rowley, was ſhewn to him,—Tuſon was obliged to ſay, that the correſpondence had not his approbation. The Admiral replied,—" had they ſucceeded in ſaving my

child, it would be juſtified, but as it was, ſuch a tranſaction made public, could only be productive of freſh pain to me, and of diſgrace to them." —And this is the light, in which the cataſtrophé muſt be viewed, by every honeſt man of feeling, all over the world. The ſtandard of truth, is neither ſo imaginary, difficult, or liable to be variouſly conſtrued, as ignorance or venality might hope to inculcate: upon ſurgical points well aſcertained, and upon thoſe fully explained, —it can be realized, to every well deſigning common capacity.

I am now to tell,—how John Hunter, when embarked in this deſperate adventure, ſteered his courſe,—piloted his veſſel thus deeply laden, —and how he brought the ſeveral commodities it contained, to general account.—How he conducted himſelf, under all his plans, from the æra of 1784, after going into this new houſe, to the cloſe of 1788, when the death of Pott, opened before him new reſources, from encreaſed practice.

The loſs of a public and able man, is ever to be deplored; becauſe the world can never ſpare him, without feeling a convulſive ſhock, whenever it be deprived of abilities,—ſuch as belonged to Percival Pott—and ſuch as were carried by him

him into honourable practice. But yet there was a conſolation which ſoothed the reflection, at the moment that it contemplated his ſudden departure.—That he had not died,—before he had been granted, many and proſperous days,—before he had ſtamped the true principles of ſurgery, on the minds of practitioners of the preſent age,—nor before he had, by the works which he had publiſhed, conveyed their ineſtimable value to poſterity. It is curious to remark, that ſuch a man enjoyed no lucrative, nor honourable profeſſional office, which was in the power of the Court to beſtow;—that as he ſought not honours, they were not beſtowed upon him;—that as he ſolicited not that which alone is honourable, by its ſpontaneouſly following merit,—ſo did he paſs through a life, with unbluſhing neglect, from every adminiſtration, to its 74th year, without it.

THE MUSEUM.

I ſhall firſt proceed, to give an account of the arrangement of the Muſeum, in this new houſe,—a valuable collection,—and of a nature rare and extenſive. If John Hunter did not form it altogether,

together, from a love for ſcience,—if it were not formed, out of an overplus of wealth,—if the lucrative poſts which he enjoyed from government, and the emoluments redundant out of practice, did not, as might have been expected, enable him to leave this valuable Muſeum, entire to poſterity, and in the manner which a pure philoſopher would have hoped,—perhaps, it might be owing to his not having calculated upon the chances of life,—as ſuch belong to wiſdom alone, as ſuch are the uncertainties of every hour of exiſtence,—and that he might be overtaken, ſooner than the plans he had arranged could have been half perfected, much leſs have been brought to concluſion. The Muſeum, as fitted up in this new houſe, was to be ſeen in all its glory,—the luſtre of which, has captivated the attention of every true philoſopher, and dazzled the eyes, and excited wonder in the minds, of the uninformed vulgar.

It does not belong to the province of any man, to interfere in this part of the appropriations of John Hunter's time, and fortune. Every perſon has the right of enjoyment in his hobby horſe, when that does not intrude nor treſpaſs upon the felicities of ſociety. And it muſt not be forgotten,—that the nature of the profeſſion of a ſurgeon, exacts from him who is deſtined

to follow it, to become conſtantly ſtationary. A ſurgeon cannot abſent himſelf for months, nor days, and again return to his occupation; and he muſt be in waiting even, when he is not, in actual employment. If John Hunter then, thus turned his attention to the collection of a Muſeum,—inſtead of building a new country manſion,—of changing paſturage, and arable into lawn,—paths, into gravel walks,—rivulets, into ſheets of water,—and down, into plantations, —his amuſement, though not of ſo general a nature, was ſurely as innocent;—and the talent, which brought the natural hiſtory of the world, into a focus, was moved to it, by ſuggeſtions of the mind, as exalted, if more rare, than that which is more commonly employed, in changing the ſurface, neceſſary, for the uſeful product of nature, into pleaſurable ſpectacle.

In my obſervations upon this Muſeum, I ſhall only contend againſt one material inference, which not only John Hunter has ſtrongly dwelt upon, but the vulgar at large have drawn from it—that it poſitively conſtituted him a ſurgeon of greater ability, than if he had not collected it, and than other ſurgeons, who had not ſuch a Muſeum. Nothing can be more incompatible with reaſon. If he made this, his leading object, other purſuits, from the nature of man, muſt

conſe-

conſequently have been the more neglected by him. But collections of this nature, or of any other, ſimilar in point of paſſion to them, have nothing to do with the principles of ſurgery. The art is in no meaſure advanced from them. For if it were ſo—and if it neceſſarily follow, —that whoever makes a collection of natural productions, muſt conſequently be conſtituted a greater ſurgeon,—the Ducheſs of Portland and Sir Aſhton Lever have been deprived of a valuable part of their reputations. The phyſician, —the painter,—and the ſtatuary, muſt have ſtudied anatomy,—but that could not have conſtituted, in either of them the art of ſurgery, without their having ſtudied, and practiſed that alſo.

In an early part of John Hunter's life, he took up the idea,—that the ſtructure and phyſiology of the human body, would never be made out clearly, but by attending to the ſtructure of animals in general. On this principle his Muſeum was formed: and it conſiſts of preparations of every part of the human body, both wet and dry; with correſponding preparations of the ſame parts, in all other animals who poſſeſs them. If an animal has a greater, or a leſſer number of parts, than the human ſubject, it is preſerved by him, on theſe accounts. The arrangement of the Muſeum is this: it begins with ſpecimens of the

the moſt ſimple, or component parts of the human body, and of the ſame parts in other animals, where they differ in ſtructure; ſuch as a muſcle, bone, tendon, ligament, cartilage, &c. To mention one inſtance; the bone from a human body, and the bone of a fiſh are very different, the one is opake and heavy, the other tranſparent and light:—theſe circumſtances will give riſe to a variety of phyſiological reaſonings.

It goes on to the more compound parts: as the heart from the human ſubject, and the hearts from all thoſe animals from which they could be procured; ſhewing the different variations. The human ſtomach and the ſtomachs of other animals: the inteſtines, the parts of generation, the liver, ſpleen, kidney, lungs, brain, in ſhort, every part of the body, the arteries, veins, nerves, and lymphatics, are ſhewn in preparations from the human ſubject, and from a variety of other animals. The bones too, of every animal that could be procured, are formed into ſkeletons.

In the arrangement of undiſſected animals, or parts of animals, of which there are a great number in the Muſeum, John Hunter has began with what he called, the moſt ſimple animal, a polype, or a leach for example, and going on

to the more compound, ends with man. The deviations from nature called, monſters, are alſo in large numbers. There is a collection of extraneous foſſils; theſe, conſiſting of the remains of petrified animals, have ſome connection with comparative anatomy: and laſtly, a good collection of calculi. He alſo poſſeſſed a fine collection of diſeaſes; but theſe do not properly belong to the Muſeum, but were uſed by him, in his ſurgical lectures.

I know of no Muſeum ſimilar to this; it may be ſaid to be, unique, or *ſui generis*; nor do I think that the aggregation or conſolidation of any former Muſeums, would have produced any thing like this: and I believe that the idea of forming ſuch a collection, originated with John Hunter.

There are ſome preparations of vegetables in the Muſeum; but I believe they are only conſidered, in an analogical way. No one, who is at all acquainted with the nature of this collection, but will readily allow, that it was the production of time, expence, anatomical excellence, and intenſe application. It is very certain, that John Hunter laid out all the money he ever got, in this and purſuits connected with it. I imagine, that beſides, what was ſent home to him, from every quarter of the globe, and which was conſtantly announced

nounced in the prints of the day, every pupil and apprentice John Hunter had, contributed to this Muſeum, more or leſs, in proportion as he was zealous, or idle.

To the formation of the Muſeum I have known, but three, whoſe ſervices are worth recording, namely, his Brother in Law, Bell, and André.—The firſt living with John Hunter, nearly the whole of the time, ſince he left ſchool, —the ſecond fourteen years,—and the third a ſhorter time.—The ſecond was, beſides, John Hunter's draughtſman, and has a ſhort time ſince, embarked for India.—The third came to John Hunter, an anatomiſt already in a ſtyle, the moſt perfected, perhaps, of the modern age.—He was bred up, in the ſchool of Watſon, and ſeemed to have purſued diſſections, and to have made preparations, purely, from the admiration of the arts. His delight in excellence was ſuch, —that he would not permit any undertaking to go forth from his hands, with the poſſibility of any ſuperiority in perfection. His neatneſs in diſſections,—his knowledge of the arts of injections and preparations,—his deep acquaintance with natural hiſtory,—and above all—his modeſt merit, and humble deportment—ſent him to do that, for John Hunter, no one, beſides, could have been found, to have done for him, and which

he could never be brought, in any large ſcale, to do for himſelf. Beſides theſe excellencies,—he was found to be neceſſary to John Hunter, in all points; and that upon ſtipulated terms, of a very inferior degree. He was his amanuenſis: and this native philoſopher was ſeen—when I went to John Hunter's warehouſe, to purchaſe his book, on the Venereal Diſeaſe,—folding up the ſheets of it, for the women to ſtitch them together:—ſuch are the ludicrous ſports of fortune. It is with infinite pleaſure,—I am able to tell—that he has found a retreat, ſecure from the perilous peltings of adverſity, as domeſtic librarian to a nobleman, whoſe higheſt characteriſtic is—to venerate virtue, and to protect merit.

Both, the departure of Bell, and André, proclaimed—that this was the æra, when John Hunter had brought his Muſeum, almoſt, to its acmé. And, as the ſubſequent part of his life was thronged with other avocations, it is to be preſumed—that the time, I have choſen, to deſcribe it, was the time, when it was moſt advancing to that pitch of perfection, which it is now to be ſeen in, as it is five years, ſince André left him.

There is an obſervation, which muſt be taken into this account, and cannot eſcape being noticed,

ticed, becauſe it leads to aſcertaining the value, in the preſent age, of wet and dry preparations, —and to place this Muſeum in a different eſtimation,—to that which was formerly annexed to thoſe Muſeums of Tradeſcant, Hans Sloane, Mead and others. The articles, both wet and dry, are found to be of a periſhable nature: and the wet cannot be preſerved, by the moſt ſtrict attention, and conſtant renewal of ſpirits,—beyond a very circumſcribed duration of time. The finer, and more minute, and delicate parts of preparations, are the moſt periſhable; the beautiful diſplay annexed to theſe, are ſoon evaneſcent; the very medium in which they are preſerved, tends to corrupt them; and that medium is alſo, very expenſive. I ſaw the preparations, belonging to Ruyſch, which are depoſited in the Muſeum at Peterſburg, going apace into decay.

As John Hunter's Muſeum derives all its value, from the ſyſtem, or arrangement of it,—ſo when parts of that are gone, the value of the remaining, will be leſſened, by the chaſm produced in it. Since the art of engraving, has arrived to ſuch a degree of excellence, and its artiſts multiplied,—theſe conſiderations are not to be ſo much lamented,—eſpecially, when it is conſidered—that by engraving, every ſingle article

article can be thus multiplied, and every phyſiologiſt can indulge his favourite taſte, by ſuch a circulation of the prepared ſyſtem of the animal œconomy. Whereas one alone, can poſſeſs the original,—and that original, from its periſhable nature, cannot laſt long. If this Muſeum be of that tranſcendant value, which the age is ſo highly diſpoſed to attribute to it,—I ſhould imagine, that it would be obtaining every point, by the whole being engraved; and this ought to have been gone about, during his life time. The ſubſcription for it, would amount to a very conſiderable ſum of money,—and the fame of John Hunter, would be thus perpetuated, upon, one, only, ſolid foundation—by his having produced a work of public utility, which the art of engraving, can make as durable as copper.—If, this, had been done in his life time, he might then have ſaid—

Exegi monumentum, ære perennius—

Soon as he was ſettled in this new houſe, he ſent out cards of invitation to thoſe of the faculty, his ſelection ſuggeſted,—to attend on Sunday evenings, during the winter months, at his

his levee; and they were regaled, with tea and coffee, and treated with medical occurrences. It would have been highly indelicate, for thoſe to have gone there, who were not invited; and it might be preſumed, as indelicate for thoſe who did go, to talk upon matters, which did not favour John Hunter's theory. Hence, every caſe, that was produced, tended to confirm, one or other of his new opinions. That gonorrhæa cured itſelf,—that ſpecific diſtance of virus was, exactly, an inch and a half up the urethra,—that every bubo could be reduced, without coming to abceſs,—that cauſtic was the only remedy, for the cure of obſtructions, and the prevention of hydrophobia,—that adheſive inflammation—ſuppurative inflammation,—gangrenous inflammation,—irritable inflammation—and ſympathetic inflammation, were no barbariſms in expreſſion,—that the blood which was the vehicle of infection, was not infected,—that tranſplanted teeth could not convey the venereal diſeaſe,—that bandages, on inflamed parts, did good,—that ſtomachs complimented death, by going directly into diſſolution, turning their contents into the abdomen,—that ſelf pollution was a very innocent diverſion—and many more opinions of high value—ſuch as that, mucus was pus, and of courſe, pus was mucus—and ſuch was the jargon

of

of this modern ſchool—and ſuch was the ſpelling book, for his brats in ſurgery.

——— He leads them, like a thing,
Made by ſome other deity, than nature,
That ſhapes man better; and they follow him,
——— With no leſs confidence,
Than boys purſuing ſummer butterflies.

SHAKESPEAR.

But leſt, the general diffuſion of theſe bleſſings for mankind—and leſt, the benefit ariſing from them, ſhould not, by this ſcheme of circulation, be fully adequate,—Another, was formed in January 1785, upon a larger ſcale of aggrandization, under the ſame roof. A ſociety for diſputation, where all were of the ſame opinion, and which conſiſted of the ſame members who viſited his levee, was eſtabliſhed. The room was called,—Lyceum Medicum,—galleries were erected around it,—the preſident wore his hat on,—and John Hunter was the patron. Here, his new opinions received, the finiſhing plaudits of approbation—and from thence, were diſſeminated fully into practice.

In this new houſe alſo,—a preſs was erected—and here, a ſynod was held, to correct his written documents: and here, he printed his book, on the Venereal Diſeaſe in 1786, the ſale of which was

was rapid, at firſt, from curioſity being artificially raiſed,—as the papers of the dayhad announced—that it was to throw all former productions, at an humble diſtance:—but, after it had been once peruſed, that ſale fell ſhort of ſanguine expectation. Why he printed at home—why he deprived a valuable profeſſion of its profit—I am not to tell;—it can only be conſidered, as one of thoſe ſteps in the ladder, that aſſiſted him in climbing, to the ſummit of his proſpect of ambition.

As he frequently had been heard to declare—that he never read books, on ſurgery and medicine—his library was barren of theſe ſubjects; it was but ſmall, and conſiſted chiefly of books, on natural hiſtory. And this was the man, who was to lay London, under contribution, for his ſurgical art.—Let thoſe conſider this, who look upon it, as a piece of art, and the maſterpiece of action,—to deceive, and make a prey of a credulous, and well meaning honeſty.—Let thoſe, whoſe foreſight allows them no other clue to direct them, ſuſpect the diſintereſted profeſſions of him, who is only ſeen in every action, ſelfiſhly proceeding in his way of ambition, careleſs of conſequences.—Let young men his admirers, feel the impulſe of riſing into eminence, by the modeſt path of genuine knowledge;—let them

adopt, for their model, the tranſcendant life of de Haller, and learn to forget, that of John Hunter;—let them ſhun the ſyſtem of popularity, acquired, by courting the little vanity of vulgar opinion; and embrace the more intrinſic ſentiments of philoſophy, which the wiſeſt amongſt men, muſt ever, irreſiſtably, venerate.

It will be found—that the advancement of John Hunter to that profeſſional height, at which he was, at length, ſeen to ſoar,—was owing to the decline of thoſe, who ſtood in his way. He did not ſuperſede, but ſucceeded to them. The death of Pott, on the cloſe of the year 1788, placed him upon a footing, equal, if not ſuperior in point of practical calls, to any living competitor. I think I may affirm—that his conſultations were more in faſhion, than any other ſurgeon's—and that his range of practice was more extenſive:—that we heard more of the name of John Hunter, than of any other ſurgeon.

There is no novelty, in remarking—that from the limited knowledge poſſeſſed by man—he is ever incapable of predicting certain events, belonging to futurity. No one can foreſee, however he may merit it, both by talent and induſtry,—what will be the ſcale of eſtimation, which he will

will be held in, by the public opinion, ten years before the time which he looks for, arrives. John Hunter had wiſdom enough—ſo far to miſtruſt the idolatry, he worſhipped, and therefore—a long while, before the death of Robert Adair,—he had obtained the promiſe in reverſion, of as many of the lucrative appointments, held by that gentleman, as he poſſibly could. In the year 1789—vacancies of theſe, and many more, offered by the death of Adair—and the offices of Surgeon General to the army, and Inſpector, were then ſucceeded to, by John Hunter:—but the influence of Keate kept from him Chelſea Hoſpital; and this was for ever after, ſeen by him, with an eye of diſcontent.

If this arrangement had been accommodated, according to the ſeparate talents of the candidates,—John Hunter ought never to have been Surgeon General to the army.—In time of peace, a man, like him, might have drudged on through the buſineſs, without much difficulty, or embarraſſment:—but in time of war, the duty requires qualities, which he never poſſeſſed;—a power of diſpatching buſineſs,—writing letters with addreſs, and diſcriminating into merit without partiality;—a juſt conception of the dignity, and honour of the office; and the perſuaſive maſterpiece, of ſo convincing any one, who aſked for

what he was not entitled, to expect,—as to induce him, by the force of well timed argument, to decline his pretenſions, without the murmurs of diſſatisfaction.

John Hunter was induſtrious, but he was ſlow; and letter writing was not in the ſcale of his education, or ability. He was biaſſed, too powerfully, to his pupils, to be publicly juſt;—as he would know no merit, from the report of any one, where he was unacquainted with, or had not educated its poſſeſſor. He ſunk the dignity, and tarniſhed the honour of the office, by the ſelections he made, and the eſtabliſhment he formed, in the hoſpitals on the Continent. He affected to be too proud to explain, where he did not mean to ſerve; and the affectation aroſe from his incapacity,—from his want of the power, of placidly giving, a decent refuſal. He arrogated a right of creating phyſicians out of apothecaries; and defied the interference, and the power of the college. He eſtranged himſelf from all intercourſe, with the corporation of ſurgeons—he was never inclined to receive their recommendation of merit; and though choſen one of the Court of Aſſiſtants, in the year 1789, he never, but once, attended in his place. He hated his equals in the profeſſion; and who can eſteem him, who hates them!!

But—

But—although the court poſſeſſed the power—yet it did not enforce it; or for his contempt, he might have been reduced to anſwer the law, inveſted by their charter. He—who would not attend his duty at the hall, nor aſſociate in the annual feſtival of harmony, eſtabliſhed by the company,—could advertiſe his name, as patron, and chairman, at the feaſt of the members of his little ſenate, the Lyceum. He was not found, to be even decent, when it interfered with his pride; and in conſultations, where he was the, laſt, called in,—if he did not like the, firſt,—he was certain to get him, diſcharged, on the ſecond or third viſit,—by ſaying, that there needed not the attendance of, two. This I am told was a favourite piece of practical revenge with him.

I am now arrived, to that period in the life of John Hunter, ſo much within the recollection of almoſt every reader,—that I ſhould be wanting in common prudence, if I were found to explain any circumſtance of his tranſactions, beyond what truth would juſtify:—and on the other hand, I never ſhould have been fitting, for this undertaking, If I withheld from the eye of the public, ſuch relations of facts, as are, by their being recent, more notoriouſly obvious. I will neither do the one, nor the other. I will

neither

neither ſcreen, nor exaggerate his conduct. Let the relations of facts ſpeak for themſelves.

In reflecting upon human nature, and watching with common attention the general operation of the paſſions,—the beſt of men are frequently ſeen,—when bowed down by oppreſſion, —diſappointed in events,—or left deſtitute of all proſpect, even, in the falſe flattery from hope —to betray a diſcontent of mind, and a jealouſy for the proſperity of others.—

Ipſe ſuum cor edens, hominum veſtigia vitans.—

In ſuch inſtances—the paſſions will flow irregularly—their ebb and their tide, will be experienced to the fulleſt extent,—and the power of reaſon will be in vain invoked, for keeping them under any degree of controul. In ſuch inſtances—the nature of their ſituation, by the operation of adverſity, upon ſuſceptible minds, is ample to explain the effect: and in ſuch inſtances—when by a happy reverſe of fortune, the contention of the paſſions has ſubſided, the boſom is again reſtored to a ſtate of repoſe, and its "Lord ſits lightly on his throne,"—when reaſon is again, in poſſeſſion of its dominion,—every lively action will proclaim, both the cauſe, and the effect.

But

But this, was not the caſe of John Hunter:—nor can I find out his apology, by this mode of adaptation. He,—from a retroſpect into the chances which were againſt him,—from his want of fortune originally,—and above all, alſo,—from his want of education,—muſt be deemed, by the higheſt expectation which might be formed of his merit, to have been in a ſituation—far beyond all proſpect of reaſonable ſuggeſtion. He had held for thirty four years, the half pay of ſurgeon upon the ſtaff, and which had amounted upon the whole, to three thouſand pounds;—he had ſo balanced his intereſt with his influence, as to ſuppreſs the alarming din, of ſerving any more;—he had found out, that another ſummer trip to Belliſle, or Portugal, could not create, another half pay, and that the honour of the ſervice was nothing, in competition with the intereſt of him, who ſerves;—he had aſſurances, that by ſtaying at home,—he could create a new intereſt, for the obtainment of higher emoluments, and in which, he at length ſucceeded. In time of war,—I ſpeak not from knowledge,—the joint offices of Surgeon General and Inſpector are ſet down, at twelve hundred pounds per annum. Theſe lucrative poſts, beſides his, purely honourable, appointments, and ſuch as were mixed with profit, as that of his being ſurgeon for eighteen years to St. George's hoſpital, were

were enough, one would have thought, to have gratified the proudeſt mind,—eſpecially as theſe were obtained, by one, who in his habit of life, was ſtrongly diſpoſed to ſolitude—by one, who has been ſaid, to find a retreat moſt congenial to his nature, in the recluſe ſtudy of natural hiſtory.

Thus conditioned,—it might have been reaſonably ſuppoſed,—that there could no longer have exiſted, an individual in the profeſſion, whom John Hunter would have viewed with a jealous ſcorn; or whoſe proſperity, he would have hoped to obſtruct. And thus conditioned —it might have been expected—that the turbulence of paſſion, the ambition for power, and the avarice for profit, would have been lulled to reſt, would have, at leaſt vaniſhed, on the approach of thoſe days of proſperity, which he had experienced, in ſo eminent a degree. But ſtrange to be told,—ſuch was not the conſequent effect. He—inſtead of poſſeſſing the placid, and tranquil countenance of a man favoured, by the world,—was found to be waging a continual war, with the ſurgeons of the hoſpital, to which he had, for ſo great a length of time, belonged. He diſputed their unanimous choice, in every freſh election. He preferred candidates, out of their regular courſe. He tried conteſt upon conteſt,

conteſt, and found his influence fail. Under the ſame intolerance of temper, he went ſo far, as to keep back, the receipts of money, paid to him, by pupils of the hoſpital, for their public attendance, at the hoſpital ;—and contrary to cuſtom, and the rule of the hoſpital, he claimed that money as his right:—for this, he ſubmitted to the indignity of being ſummoned to the board, where his plea was heard, and he was forced to refund.

He was not likely, from his native diſpoſition, to yield up points hereafter, in conſequence of the loſs of influence he had already ſuſtained ; and which, the experience of conteſts had already diſcovered. And therefore—I have every reaſon to be perſuaded—that, for the three laſt years of his life,—he never approached within the diſtrict of the hoſpital, without its affecting his mind, and diſordering his whole frame. In one of thoſe diſputes, at the hoſpital—where the moſt trifling addreſs might have eaſily turned aſide, the heat of altercation,—his powers were ſeen, in the very act, to give way, he fainted—and inſtantly expired—

Frigidus obſtiterit circum præcordia ſanguis—

This happened at the hoſpital, on Wedneſday, the 16th of October, 1793, and when, he was in the 64th year of his age.—He was carried to his houſe, in Leiceſter ſquare—in a cloſe chair belonging to the hoſpital—and was interred, on the Wedneſday following,—in the public vault belonging to St. Martin's, a few ſelect friends attending, at his funeral. On being told of this event, on the ſame day, I recollected having ſeen the bay ſtone horſes returning, through Piccadilly, home, without their maſter,—and this circumſtance introduced to my reflection—the ſympathy which Virgil has attributed, to the war horſe of young Pallas, in his funeral proceſſion—

Poſt Bellator Equus, poſitis inſignibus, Æthon
It lacrymans—

John Hunter could never ſeparate the loſs of Chelſea hoſpital, from the perſon of Keate,—nor, would he permit himſelf to reflect,—that if he had not the influence, for obtaining what Keate had,—and what, if Keate had not obtained,—another might;—and that, whoever had ſucceeded to it, would have been equally obnoxious to him, as long as, ſuch a malignant impreſſion remained, upon his diſtempered mind.—With Gunning, he had been acquainted, more than thirty years; and notwithſtanding he had made, man, the ſtudy of his life, that book of nature,

ture, muſt have been blotted on the page, which could have informed him,—that Gunning would never tamely ſubmit, to a perſonal inſult, from any one. To Walker, he had been well known only a few years, leſs; and he muſt have evinced a ſuperiority, in the art of tormenting, above all competition,—when he urged his good nature to enmity. He certainly misjudged his own power, —when he broke a lance with Keate—in trying to keep him out, of St. George's hoſpital. Beſides the mortification he experienced, from finding, that Keate's perſonal influence was ſuperior to his own; he brought another proof fatal to his ambition, to the teſt;—that, in the conteſt, almoſt the whole of the reſpectable part of the faculty, in Weſtminſter, were againſt him.

By turning ones attention away, from theſe effects,—and ſeeking for a cauſe, to account for them, through the aid of reflection,—I think it not improbable, but that the whole of John Hunter's irritable conduct, and particularly within theſe laſt four years, might be attributed to the nature of that diſeaſe, which had been progreſſively increaſing—and which, at length, was found, thus abruptly, to have been the cauſe of his death. He had long complained of a palpitation, about the region of his heart; and inſpection has ſince confirmed, that—as it was

ſuſpected—the veſſels were gone into a degree of oſſification. Some years ſince, he was induced to go to Bath, and try the effect of the water, there, —from thence he returned, ſomewhat refreſhed indeed by purer air, but without the cauſe of complaint being removed. Such an interruption in the circulation of the blood, which conſtitutes the firſt principle of vital power,—muſt have phyſically operated—by producing in an exacerbated degree, very irritable ſymptoms on a mind—ever too prone to that indecorous propenſity.

When it is conſidered that—before his appointment to the office of Surgeon General,—every minute of the day had from him, the ſtated allotment—and that in ſome degree, regulated by his choice, the ſtate of his health, and natural powers; and when it is alſo conſidered—that he was,at an age beyond ſixty—thus harraſſed by diſeaſe—reduced to forego ſome of his moſt favourite purſuits, in exchange for the toilſome taſk of new meaſures, in an office of buſtle,—one can eaſily be brought to think—that the palpable diſeaſed irritation of his mind,exacts all the apology—which humanity founded upon reaſon, can beſtow.

I believe

I believe John Hunter to have been, one of the moſt induſtrious of men. The way in which his time was devoted,—before he obtained the public appointments,—was, as follows:—He roſe very early in the morning, and went immediately into the diſſecting room,—where he ſometimes diſſected, and gave directions concerning, what he would have done, in the courſe of the day. After breakfaſt, he attended to thoſe patients who came to his houſe. At eleven he went abroad; and was employed in viſiting patients,—attending at the hoſpital,—and when the occaſion called for it, in opening dead bodies. He eat very hearty at his dinner,—and rarely drank more, than a glaſs of wine, and ſometimes not that. In the evening, he was engaged in reading his lectures, and writing down obſervations, which he had made through the day,—or preparing,for the next coming publication. He ſeldom retired to reſt till twelve, or one o'clock.

His perſon was about the middle ſtature: he was rather robuſt, but not corpulent: his ſhoulders were broad and high, and his neck remarkably ſhort: by the exertions—which he conſtantly made, after the manner of ſomething like a cough,—he ſeemed as if he ſolicited,—to ſet the circulation of blood a going. His features

features were hard,—cheeks high,—eyes ſmall and light,—eye laſhes yellow, and the bony arch protruded. His mouth was ſomewhat underhung. He wore his hair curled behind. His dreſs was plain, and none of the neateſt. He was frequently ſeen to ſmile in converſation—but it was generally provoked, from a ridiculous, or a ſatirical motive.

I am to confeſs—that in my account of him—his virtues have appeared ſomewhat thin, and ſhadowy:—but throughout his tranſactions, as well as his papers, I cannot accuſe myſelf, of having paſſed a ſingle virtue by,—but have given the ſcatterings I have found, the ſtrongeſt impreſſion they could bare.—It is for his admirers, to bring forth—with all their force—fruits praiſeworthy of their venerated Patron;—eſpecially, as the gentle ſpirit of philoſophy was never ſeen, to pervade his public conduct. It was not in my power, to produce more inſtances of the philanthropy of John Hunter—than were to be found, in the public relations of his life;—but although I have no authority to reaſon upon, but facts before me,—I am not from that cauſe diſpoſed to conclude—that he was more deficient in all the ſocial virtues, than another; and will readily ſuſpend my judgment, until the documents of them have been produced by his admirers.

admirers. Men frequently play characters abroad, and repreſent themſelves at home. Virtue and vice are both engrafted, intimately, with human nature:—whichever of them is moſt predominant, will compreſs the other, and conceal it, from common obſervation.—It is, the buſy zeal of a prying eye, which can alone diſcern, more than will be apparent,—upon the ſurface of every public character—and, that, of either extreme: David is known, to have been a man after God's own heart, yet, this pattern of excellence,—this paragon of virtue—has been diſcovered in a ſituation—tripping into vice: and Walpole has brought forward—anecdotes of virtue—and combined them with the hiſtory of Richard, the Third.

In many of the criticiſms, perhaps, I may be told, that I have been over-nice: but my authority is ſtrong. "What," ſays Johnſon, "is borrowed, is not to be enjoyed as our own, and it is the buſineſs of critical juſtice to give every bird his proper feather."

Let fiction ceaſe with life, and let us be ſerious over the grave.

FINIS.

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